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SECTION OF GEOLOGY AND MINERALOGY

APRIL 3, 1944

DOCTOR HARLAN T. STETSON, Massachusetts Institute of Technology, Needham, Mass.: *Modern Evidences for Differential Movement of Certain Points on the Earth's Surface.* (This lecture was illustrated by lantern slides.)

The subject of our discussion is one of those problems that not only straddles the border line between departments of knowledge, but transgresses far into the adjacent territories of several sciences.

Making no pose as a geologist, however, I must confess that what I may have to say in regard to physiographic examples of differential displacements in the earth's crust will have to be from such little knowledge as I have been able to borrow from geologists, and I venture to proffer such second-hand information only on the ground that we have a board of experts among us to whom questionable points may be referred subsequently. My excuse for venturing upon this subject comes from a very vital interest that must concern every student of the earth.

Certain investigations, with which I have been more or less engaged for the last fifteen years, seem to lend promise that the problem of lateral shifts in the earth's crust may be helped toward solution by the continued observation of the latitude and longitude of certain presumably fixed points on the earth's surface. In the American Geophysical Union, that unique organization of students of the earth-sciences, ex-

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perts from many different but interrelated fields commingle freely. It would appear that it was through such commingling that some astronomers became contaminated by some geologists, perhaps with a fond hope that a little contamination of astronomy with geology might be of mutual benefit.

The most dramatic evidence for translational displacements in the earth's crust are still to be found in the geologists' picture album, and I am indebted to our colleague, Professor Thom, for the loan of certain photographs that portray, graphically, displacements of many feet along fault lines familiar to every geologist. Not all of these displacements are sudden occurrences, such as accompany great seismic disturbances. The slow crawling of the crust in the neighborhood of the San Andreas fault is well known to the petroleum industries, whose pipe-lines have bent and buckled and often broken under the stresses and strains of the earth's crust. I have inquired of my geological friends for evidence in other parts of the country of equally striking displacements,—thus far without very encouraging results. It would appear that California, in keeping with her other superlatives, may continue to boast not only as the state par excellence of Hollywood stars and gigantic telescopes, but also of maximum terrestrial displacements.

Of course, looking back into geologic history, we have a panorama presented of a long series of depressions and upheavals of overthrusts and underthrusts which have caused the characteristic formations of the orogenic zones. Dr. Bucher has stressed the role of thrust-sheets in orogenic deformation and would cite as a specific example the Cumberland thrust-block lying across the tri-state corner of Kentucky, Virginia, and Tennessee.[1] Here, a slice of rock over 120 miles long, some 20 miles wide, and a mile in thickness, has been sheared off closely parallel with the bedding, and pushed along a distance of at least 7 miles. In addition, the whole slice has apparently been rotated, producing cross wrinkles.

The presence of great rift or shift zones in continental shield-areas was graphically depicted by Dr. A. W. Jolliffe,[2] at a recent symposium, as follows:

"Many faults in the Canadian Shield are known to follow essentially rectilinear vertical fractures that extend for distances up to several hundreds of miles. None of them is known to be marked by major vertical displacements whereas a number show strike-slip movements amounting to several miles. From this it is inferred that the fault-movements were dominantly horizontal of the same general

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nature as those along the San Andreas Rift. Movements along many of the Shield faults were recurrent, the most recent being late Pre-Cambrian or even younger in age."

While geologists generally recognize that large differential horizontal displacements have taken place in the past, probably many still believe that such movements between crustal segments are negligible at the present time. Professor Thom,[3] in a recent report of his special committee on the Geophysical and Geological study of continents, stresses that "Evidence has now accumulated to a point where it is reasonably clear that an appreciable differential crustal shift is still going on in different parts of the world. And it, therefore, seems highly expedient that adequate data as to nature, magnitude and disposition of these relative movements should be obtained forthwith because of the critical bearing of these data upon over-all geodetic and cartographic programs and operations."

It seems to be beyond contention that a certain recurrence, at more or less definite intervals, or a cyclical rhythm in large crustal displacements, has taken place in geologic time. Accumulating evidence continues to point that such movements must still be taking place; but when large masses of terrain are involved, especially in regions unscarred by fault lines, there is a difficult problem in determining either the direction or the amount of such movements as may be taking place currently. It is for this reason that the geologists look to the astronomers to see what evidence there is, if any, that displacements have taken place in very recent times. To interweave more closely the determination of the crustal movements with the astronomical method of finding changes in position, we perhaps should remove ourselves sufficiently far from the earth itself to get the cosmic pictures of the planet with which we are concerned, and its immediate environment.

To use an analogy, those of us who inhabit the earth find ourselves aboard a ship on the high seas of space. Contrary to the opinions of the earlier astronomers, this spherical ship is far from fixed. At this very moment, we are being carried eastward with the earth's daily rotation at a velocity, in this latitude, of about 600 miles an hour. Meanwhile, the good ship Earth is steaming ahead in its orbit about the sun with a velocity of 20 miles a second. The sun, however, which is the flagship of our fleet of planets, is steaming ahead on its course among the nearer stars at a velocity of some 12 miles a second, or 40,000 miles an hour. The sun, in company with other suns comprising a still larger

squadron, is, in turn, sailing through space at a speed of several hundred miles a second toward the lights of other celestial ships seen at the limit of the horizon of our greatest telescopes. Meanwhile, as passengers aboard the earth, we are being tossed to and fro, all quite unconsciously, due to certain peculiar wobblings of the earth's axis, and now even the deck of the ship on which we stand—the crust of the earth—proves itself unstable as it trembles now and then, due to internal catastrophes within the ship's hold.

As passengers aboard our frail celestial craft, we are subject to all the cosmic forces, known and unknown, that play about in the mysterious ocean of space. Only as we examine our environment, can we hope to come to anything like a complete knowledge of some of the disturbing forces that are operating upon the earth. It is to astronomers, as the navigators upon the bridge of this strange ship, that our fellow passengers look to tell the ship's position, define her course, and speed. Only by observations of those lights exterior to the earth—the distant stars—can we hope to evaluate not only the motions of the earth but movements that may be taking place in the earth's crust. While, in the last analysis, we can never hope to define the absolute motion of ourselves among a vast system of moving stars, we can take courage at the degree of precision which may be attained from astronomical observations. For those not intimately acquainted with the applications of practical astronomy, it comes perhaps as something of a surprise when we consider that it is possible, by looking at the stars, to determine one's position on the surface of the earth with an uncertainty of but a very few feet. More specifically, from a series of careful observations, we can determine, within a foot or two, where we are north and south (latitude) and, in the east and west direction (longitude), within an uncertainty of perhaps three or four times that amount.

The problem of the determination of latitude is fundamentally very simple, since an observer is as far north of the earth's equator as his zenith appears from the celestial equator, the zenith being determined by the direction of the plumb-line at a given point. One needs only to attach a telescope, in a sufficiently expert manner, to a level bubble and the zenith is located among the stars, whose positions have been catalogued with respect to the celestial equator. Unfortunately, however, the positions of the stars can be determined only if the latitude of the observatory is known; so that it is only through a series of successive approximations of a large number of cooperating observa-

tories that the positions of the stars can be known so that the latitude of any station can be absolutely determined.

VARIATIONS IN LATITUDE

For finding small variations in latitude at a single station, the true value of the star positions is not necessary, provided the star has the courtesy to remain stationary during any series of latitude observations in which we are interested. From a long series of observations made at a selected number of stations operated through international cooperation along the 39th parallel, it has been possible to evaluate, with great accuracy, small variations in latitude. It has been determined that the pole of rotation of the earth is constantly shifting within the earth itself. This wandering of the pole, first announced by Chandler and later by Kustner of Berlin, is of a somewhat cyclical nature, having a period of approximately 428 days, with a superimposed annual component. The migration of the earth's pole of rotation about the pole of its figure has never yet been observed to exceed a radius of 30 feet. An inter-comparison, however, from time to time, of the latitudes of the several stations involved, namely, Gaithersburg, Carloforte, Chardjui, and Mizusawa, have revealed certain discrepancies that may well have indicated crustal displacements. In the reduction of the last International Latitude Survey, Kimura[4] bluntly stated that, with the position of the pole determined from all the other latitude stations excluding Mizusawa, the latter station could be considered to have moved southward by about 3 meters from the position occupied ten years earlier. On the other hand, certain geodesists are loath to accept this as evidence, proferring rather the alternative interpretation that the discrepancy lies in errors in the adopted position of the stars used in the reduction of the observations. With a certain daring impishness, however, I venture to remark in paraphrase "The trouble, dear Brutus, may not be with our stars, but with ourselves, that we are moving."

It should be emphasized that, in determining latitude, we are concerned with the precise location of the zenith among the stars. Anything that influences the direction of the plumb-line, peculiar to the observatory station, will show up as a variation in latitude. A shore station, subject to the loading of the ocean tides, will have its plumb-line periodically deflected in the course of a lunar day, by the gravitational attraction of the rising tide of water off shore. The tilting of the

land which results from the hydrostatic pressures will be readily manifested on a tilt-meter, but this tilt will not displace the direction of the plumb-line, nor cause a variation in latitude, unless there is a lateral shift along the equipotential surface. The distortion of the plumb-line due to the attraction of a mass of ocean water off shore has been worked out and can not much exceed one one-hundredth of a second of arc. A tilt of the coast line, due to the loading, may amount to a considerable fraction of a second of arc for some distance back of the shore-line itself.

The moon, of course, causes a calculable tide in the solid earth as well as in the oceans. The celebrated Michelson-Gale experiments, carried out on the ground of the Yerkes Observatory in 1913 to 1915, made possible the observation of well-defined earth tides through a tilt-meter consisting of a horizontal pipe 500 feet long, half filled with water. The microscopic tides caused in this pipe were measured by an elaborate optical method, and revealed amplitudes in the waves less than those calculable on the assumption of an unyielding earth. The discrepancies between the observed and the calculated values of the tides made possible the evaluation of the amount of yield in the solid earth. From the results, it was found that the rigidity of the earth, as a whole, was perhaps a little greater than that of steel.

It should be pointed out in this connection that such a calculation must, of necessity, be made for the earth as a whole as a homogeneous body. Were there a larger yield in the crust than in the earth's interior there would be no way of determining it by this method. Again, it should be remembered that the observations revealed tilts only. Were there an actual rhythmic translation of the crust, as the tidal wave passed the instrument, this could not have been detected by the water pipe. On the other hand, any horizontal displacement or movement of the superficial crust along the equipotential surface under the influence of the tidal force would cause the plumb-line to shift its position, and a variation of latitude would be detected by the zenith telescope.

It was in the interest of trying to discover whether or not such tangential displacements as might take place could be detected by astronomical observation that I spent some years in investigating latitude observations for any variation that might appear to correlate with the moon's position. The results of the investigations indicated what appeared to be a definite lunar correlation in the Gaithersburg series, made

with a photographic zenith tube in 1913–1914.[5] Further studies revealed similar correlations for the observations made at Ukiah. The amplitude of the variations found were of the order of several hundredths of a second of arc, corresponding to lateral shifts from two to five feet in the north and south direction.

These results were considerably larger than those that would be anticipated on the assumption of an earth tide calculated theoretically for the earth as a whole. Others, who have made a rigid harmonic analysis of latitude observations for a lunar tide, found results consistent with the theoretically calculated earth tide. It should be remarked, however, that, in my study, the form of the curve of latitude with the altitude, or time of lunar day, has by no means repeated itself completely in successive years. Patterns differ both in phase angle and in amplitude. Such results would not be inconsistent with tidal forces encountering resistance to deformation. Such an effect could be completely masked when several years of observation are subjected to a rigid analysis that pre-supposes symmetric harmonics.

It is of particular geophysical interest to note that the stations conveniently selected by the International Union for determining latitudes happened, with one or two exceptions, to lie in orogenic zones. The stations originally selected were Gaithersburg, Maryland; Cincinnati, Ohio; Ukiah, California; Mizusawa, Japan; Chardjui in the Himalayas; and Caloforte, off Sardinia.

Observations at the Cincinnati observatory were early abandoned because of discrepancies and large probable errors in the results. The Gaithersburg observatory was next abandoned for a time, but, after a lapse of several years, has again, fortunately, been reoccupied. The location of the other stations are, without exception, in extraordinarily interesting sections geologically, and perhaps could not have been better placed if the astronomers had been hunting for discrepancies that could be caused locally through crustal displacements. The astronomers, on the other hand, are naturally reluctant to attribute latitude discrepancies among the several stations in question to differential displacements. The object of the latitude project was patently to observe the motion of the earth's pole of rotation, rather than to try to discover crustal movements in the neighborhood of the telescope, which they presumably believed fixed on concrete piers where they were intended to stay put.

I make one further comment on this problem of latitude before dis-

cussing the East and West coordinate of longitude. Some twenty years ago, Dr. Lambert, of the Coast Survey, presented an exhaustive discussion of the latitude observations at Ukiah, California,[6] in which he showed that, during the decade and a half from 1900 to 1915, the latitude of Ukiah had more or less steadily increased some 0.15″. For some reason, it seems easier for astronomers to allow the imaginary earth's pole to slip within the crust than to assume that the actual territory surrounding Ukiah had, during this interval, inconsiderately slipped north 15 feet; so the interpretation of the result at first was that the North pole of the earth had been slowly migrating toward North America. This did not appear plausible, so it became more or less the consensus of opinion that the trouble, after all, was with the stars—that they were drifting. By arbitrarily correcting the declination of the stars used for observation, one could make the earth's North Pole behave more seemly, and thus not excite the Californians over an unwarranted skid toward the North Pole. That, at least, placed the chief worry in the hands of the astronomers, and relieved the geologists and real estate agents of any cause for alarm. Who, other than the astronomers, cares especially whether the stars are drifting or not? We can keep that secret within the family. However, more seriously, even some astronomers, in off moments, have geological interests, and it is coming to be recognized that, while it is difficult to prove, or disprove, some translational movements in the region of the latitude stations, some of us are advisedly considering other methods of observation that do not pre-suppose a knowledge of the absolute declination of stars, that, in turn, must of necessity be found only from assuming values for the latitudes of the observatories from which the observations are made! The only apparent method of accomplishing this is the observation of stars above and below the pole. This can be well done in high latitudes. Unfortunately, on account of atmospheric refraction, one cannot expect the degree of precision in such a method as is attainable by the adopted Talcott Method of zenith observations.

The possibility of comparing latitudes of certain well-established stations after an interval of 70 or 80 years interested Dr. Frank Schlesinger, late Director of the Yale Observatory, and I quote from a letter written by him to the Director of the Coast and Geodetic Survey on May 5, 1930:

"As you are doubtless aware, the problem of variations of latitude has recently taken on a new aspect of interest, owing to the possibility of a secular change, as

well as periodic changes, in the position of the pole. This is one reason why the resumption of observations at Gaithersburg has become so important. An independent way in which considerable light can be thrown upon this question occurred to me several years ago, and I have discussed it with a number of my colleagues, including Doctor Bowie, chief of your division of geodesy. The plan is to reoccupy for one or two nights each, a number of the stations at which observations for latitude were made 20 or more years ago by the Talcott method. The same stars should be observed now as were used then. From the results obtained in this way we may reasonably expect light to be thrown upon the following questions: (1) The actuality of a secular motion of the pole; (2) the possibility of slow continental, or at least general drift; (3) a better determination of the systematic errors in our adopted systems of proper motions of the fixed stars."

As a result of this letter, three stations in the United States, whose latitudes had been well defined, were selected for reoccupation: Mt. Pleasant in Maine, Mt. Tom in Massachusetts, and a station in Des Moines, Iowa. The latitude of these stations had been accurately determined in 1851, 1862, and 1869, respectively. In all cases, the Talcott method of the Zenith telescope had been used. The results of this reoccupation of these stations showed that, using the same stars and the best available star positions from the well-known Boss catalogue, Mt. Tom had decreased in latitude by 0.99″; Mt. Pleasant in Maine showed an increase in latitude by 0.29″; Des Moines, Iowa, showed a decrease in latitude by 1.49″.[7]

Translated into linear distances, the evidence showed that, in the last three-quarters of a century, Mt. Tom had slipped about 100 ft. south; while Mt. Pleasant in Maine had gone north about 30 ft.; and Des Moines, Iowa, indicated a southerly drift of 150 ft., with respect to the earlier position.

The Lick Observatory, Mt. Hamilton, California, is one of the few remaining major observatories that has continued positional astronomy. An analysis of the latitude of this observatory, based on meridian circle observations from 1893 to 1918, was made some years ago by Professor Lawson and published in the bulletin of the Department of Geology of the University of California.[8] Professor Lawson's discussion exhibited evidence of an increase in latitude of 0.40″ from 1893 to 1903. In 1903 a conspicuous drop of 0.63″ in the value of the latitude occurred, an interval coincident with the earthquake of August 2, 1903. The somewhat more scattered observations from 1903 to 1918 showed a subsequent increase in latitude, so that, by 1918, the position of the observatory was essentially the same as at the beginning of the series. Dr. Campbell, then Director of the Lick Observatory, while considering that small changes in latitude may have taken place at about the time

of the earthquake, believed that a certain amount of the change pointed out by Professor Lawson could be accounted for by possible accidental errors. The otherwise apparent steady northward drift of the Lick Observatory is quite consistent with, though somewhat larger than, the increase in the latitude of Ukiah, and, I might also add, of Gaithersburg, which Dr. Lambert of the Coast Survey has discussed at length. During the earthquake of 1906, when dislocations of six meters were reported, no definite proof of a change in the position at Mt. Hamilton was found from Lick Observatory observations. It is surprising that, from triangulations made before and after 1903, the report was made that no certain evidence existed for a shift in the position of the Lick Observatory.

VARIATION IN LONGITUDE

Fortunately, when we come to the consideration of detecting movements of the earth's crust in an east and west direction by astronomical observations, we do not have to be concerned with the absolute position of the stars. The difference in longitude between two places is nothing but the equivalent of the amount of time that elapses between the moment when a star is seen on the meridian of an observer at one station, and the moment when it is seen on the meridian at a second station west of the first. If an observer at the Naval Observatory in Washington learns, by wireless, the exact instant when a star has been observed as it crosses the meridian of Greenwich, and finds that 5 hours, 8 minutes and 15.78 seconds have elapsed before it crosses the meridian of Washington, he knows that the longitude west of Greenwich is 5 hours, 8 minutes 15.78 seconds; or an equivalent in arc of 77° 0′ 3.7″. If, from time to time, this interval should vary in the last decimal place, he might suspect, if he is mobile minded, that the longitude between Greenwich and Washington varied.

While in the problem of differences of longitude we have delightfully dismissed the problem of accurate knowledge of star positions, we have introduced yet another pernicious uncertainty. This is the degree of accuracy with which we can call up London and find the exact moment of the meridian passage of any given star. Of course, in practice, many stars are observed on both sides of the Atlantic, and the best possible values that such observations allow are made for the corrections for the Greenwich Observatory clocks. The exact time is then broadcast at stated intervals, by radio, which can be picked up by our Washington

observatory and compared with the Washington time of transit of the same stars. Meanwhile, however, we have had to rely upon some mechanism that would keep accurate time during the five-hour interval from the moment when the stars are observed in Greenwich and the time when the same stars can be observed in Washington. Fortunately, we now have rather good timepieces, which can be trusted with an astonishing degree of reliability. In the old days of longitude observations, time signals had to be passed through the Atlantic cable, and large uncertainties in the reception of time signals from England were inherent in the manner of the electric performance of the cable. Time signals can now be exchanged by radio waves, which are commonly assumed to travel with the velocity of light. Allowing for all knowable uncertainties, one may state, without exaggeration, that time can be determined on both sides of the Atlantic and signals exchanged, with a probable error of only $\pm$ 0.007 seconds.

A few years ago, Mr. Alfred Loomis and myself became interested in what appeared to be unaccountable variations in time observed on both sides of the Atlantic—variations many times larger than any errors of observation or transmission by radio would seem to allow. Such variations, furthermore, showed a more or less rhythmic oscillation over periods of days and weeks. At that time, we felt that there was fairly good evidence that the position of the moon is definitely related to small shifts in latitude. We therefore undertook to see whether or not the small differences in determination of time on both sides of the Atlantic, that might represent small variations in longitude, could also be attributable to the moon. The presentation of the first paper on this subject to the American Astronomical Society[9] at a joint meeting with the American Association for the Advancement of Science at Atlantic City, in 1932, so startled the "New York Times" that its front page, the next morning, startled the authors, the astronomers, and the rest of the world with the idea that the two English-speaking people on the two sides of the Atlantic were sometimes nearer to each other by 60 feet, and sometimes farther away from each other by 60 feet, dependent upon what the moon had to say about it. Additional investigations by Mr. Loomis and myself, and later published by the Royal Astronomical Society,[10] seemed to corroborate our earlier findings. The variation in time which appeared to correlate with the hour angle of the moon, or the hour of the lunar day, represented recurrent discrepancies of about five hundredths of a second of time. If interpreted as a lineal change

of distance in the east and west direction, this would correspond to a range of 64 feet. While we were cautious in not forcing any geological interpretation as an explanation for the phenomenon, we could not but speculate as to what hypothesis could be found to explain sanely a to and fro movement of the earth crust, were such really to exist. Much water has gone over the dam since this was first published. As to this hypothesis, there have been, geologically speaking, periods of submergence and emergence, of inundation and denudation. I think I can very briefly summarize what has happened since.

I regret that it is not possible here, on account of time, to present more details of the evidence which Mr. Loomis and I presented for a correlation between the position of the moon and the suspected variation of longitude. I should emphasize that we called attention to the fact that our table showed a marked seasonal variation about which it was remarked we should have more to say later in another paper. We did, however, publish, date by date, the reduction of the time signal observations upon which our findings were based, in the hope that others, challenged by the problem presented, might be interested to make further contributions.

We did not have long to wait. A Japanese investigator, Kawasaki,[11] utilizing our tabulated data, soon took the floor and showed that the reversal in phase of our curve of longitude variations, which we had attributed to the change in the declination of the moon from the north to the south side of the equator, could be accounted for by an annual term of unknown origin; this being the seasonal variation in the values of longitude variation to which we had called attention.

We now know that Kawasaki was wholly right in showing that, under the conditions of which the star observations were made, such an annual term could produce the effect observed, so far as the reversal of phase in the sine curve was concerned. He made no attempt, however, to account for the occurrence of the annual term or seasonal variations, or to deny that such an effect could be produced by the moon.

Shortly after the publication of Kawasaki's paper, Professor Schlesinger, after correspondence with me, undertook to make the necessary calculations to see if the known variation in the position of the pole, that caused the annual term in the variation of latitude, could not introduce an appreciable variation in longitude for geometrical reasons. Professor Schlesinger's deductions were published in 1937, in the Monthly Notices of the Royal Astronomical Society,[12] and did indicate

that a large part of the variations in longitude, that we had attributed to changing positions of the moon, could and should be accounted for on the basis of this annual term. Since that time, I have corrected all our previously published data by applying this annual term. Though not yet published, the resultant curve still persists in showing a maximum and minimum change in longitude across the Atlantic with the hour angle of the moon. This shows in the exchange of time signals between the Naval Observatory and both Greenwich and Paris, but not across the English Channel. The magnitude of the variation with the lunar day, however, is reduced in amplitude by the correction to about one-half the former value assigned by Mr. Loomis and myself. If the remaining variations, therefore, are translated into linear values, there remains a variation of about 32 feet to be accounted for in the distance across the Atlantic, that correlates with the position of the moon.

To add zest to the controversy, a publication was shortly forthcoming from the Jesuit Observatory at Zi-ka-wei, China, showing that a study of the intercomparison of time signals between Shanghai and the European stations at Nauen and Bordeaux revealed periodic variations in longitude between Berlin and Zi-ka-wei, dependent upon the moon's position. The explanation advanced was an earth tide causing a linear variation in distance of 60 feet between central Europe and the east coast of China. The amount of the variation given was several times larger than any probable error of the observations.

Unfortunately, after June 1934, shortly after the publication of our original papers, the daily intercomparisons of the 17.6 Kc. transatlantic time signals between our own Naval Observatory and the observatory at Greenwich ceased, due to changes in the transmitting station at Annapolis. We were obliged, therefore, to try to continue this study through time signals on short wave-length or high frequencies. The short wave transmission, however, across the Atlantic is so unreliable, particularly during the summer, that the project had to be abandoned until the 17.6 kilocycle frequency could be re-established. Through the coöperation of the American Geophysical Union, and Captain Hellweg, Superintendent of the Naval Observatory, the time signals were resumed on the 17.6 kilocycle frequency in 1939. However, hardly had the interchange of time signals on this reliable wavelength been re-established before the European War broke, and we have since been deprived again of this source of data.

Meanwhile, an exhaustive study was begun at the Naval Observa-

tory, to see if any lunar effect in time observations could be detected from observations made at Washington with the photographic Zenith tube. The results of this series of investigations seemed to show that observations from March 1934 to February 1939 showed a small correlation with the moon of only such magnitude as to be consistent with an earth tide calculable on the basis of the accepted constants for the earth's rigidity. If apparent variations in longitude from time comparisons on two sides of the Atlantic were due to earth tide deformation, one would expect that results at either of the two observatories concerned would show a certain dependence of meridian observations upon the hour angle of the moon. However, it should be stated that, between the date of publication of the paper by Mr. Loomis and myself and the date at which the observations at the Naval Observatory were studied, our intercomparison of the time signals showed a decadence in the amplitude of the "lunar effect."

Since the complete abandonment of the 17.6 kilocycle transmission, no further study could be made. Meanwhile, other computers at the Naval Observatory have investigated latitude observations made at Washington and have found no variations with the moon, other than what could be accounted for on the accepted theoretical earth-tide basis. I venture, however, to continue to stress that, in any rigid harmonic analysis of a long series of observations, only those terms will persist that show a uniform rhythmic oscillation throughout the whole period of time in which the data were collected. Such a method of analysis does not allow for the possibility of an intermittent effect, such as might occur were deformation irregularly resisted, as might well occur in creep phenomena. The last chapter in the discussion of this problem does not yet appear to have been written.

In arriving at our earlier published conclusions, Mr. Loomis and I pointed out that, in interpreting the variation in time signals exchanged across the Atlantic, we had tactically to assume that the speed of the radio waves carrying the signal was the same in passing from west to east as in passing in the opposite direction, and that the velocity of the wave does not substantially change during the interval covering each day's exchange of signals,—an assumption which should be reasonable, but which can not indefinitely pass unchallenged. Experiments for the verification of such an assumption have already been planned[13] and should be carried out after the termination of the war.

May I now call your attention to such evidence as exists for two of

the most astonishingly large changes in geographical coördinates that have ever been published from the study of the intercomparison of astronomical determinations? One of these concerns Greenland and the other South America.

In the summer of 1932, a Norwegian, Hans Jelstrup,[14] conducted an expedition to Sabine Island, in the Melville Bay region, off the west coast of Greenland. The purpose of his expedition was to reoccupy a station whose latitude and longitude had been determined by Börgen and Copeland in 1867 and 1870. Jelstrup identified the identical pier used by the observers in 1870. The results of the new determination gave the position of this pier as 615 meters west of the position given by Börgen and Copeland in the 1870 observations. The comparison of the longitude determined by Jelstrup with that given by Börgen and Copeland in 1870 has been questioned on the grounds of inaccuracy of the earlier observations; and Jelstrup's conclusion that, in 1932, the station was 615 meters west of the earlier position, has been warmly challenged.

It is true, that Börgen and Copeland did not have the advantage of the modern radio for time comparison, nor the use of a cable for receiving Greenwich time. They were therefore obliged to depend upon lunar culminations and observations of occultations for determining Greenwich time. These methods are well recognized as quite unsatisfactory and lacking in precision, compared with the telegraphic or radio method. It is of interest, however, to find just what degree of reliability could be placed upon those early determinations. For this reason, I have taken the trouble to examine with considerable care the original detailed publications of the 1870 expedition.[15] Their probable error was about plus or minus 1.1 seconds for either method. The longitude determinations from the four-star occultations agree within 1.5 seconds of the longitude found from the sixteen lunar transits. This is as good an agreement as could be expected in the case of the methods used.*

The uncertainty of Börgen and Copeland's observations of longitude, expressed linearly, amounts to about 80 meters, while the modern observations of Jelstrup in 1932 could represent an uncertainty of not much more than 20 meters. Now, if we allow for the worst possible combination of these estimated errors, it would appear that the results

* Observations were also made by Green in 1870 to determine corrections to the moon's position, thus minimising uncertainty due to this possible source of error.

in 1932 could be expected to agree with the 1870 observations within about 100 or, at least, 200 meters, whereas the position actually found by Jelstrup was indicated to be 615 meters west of the position determined from the earlier measurements. The conclusion seems to be that, in the 62 years interval, Sabine Island had moved west by at least two or three hundred meters.

Critics of this interpretation should bear in mind that there is also evidence of movement from the comparison of the latitude determinations made in 1870 with those made in 1932. Jelstrup's value for the latitude was 3.2″ less than that found by Börgen and Copeland in 1870, an amount which indicates that the pier, as was occupied, had moved 100 meters south of the 1870 position. Whatever doubt may be cast upon the interpretation of the longitude differences, it should be emphasized that the latitude determinations were entirely independent of the exchange of time signals, and that the order of accuracy attained for latitude determinations in 1870 was probably nearly as good as in 1932. The uncertainty involved would appear to rest chiefly upon the question of the positions of the stars used. The conclusion of the Börgen-Copeland and Jelstrup expeditions would seem to indicate that Sabine Island had drifted south and west some seven hundred to eight hundred feet during the 60 years that had elapsed between the expeditions.

Now, for the other startling incident! In 1931, a redetermination of the longitude between the Argentine Observatory at Cordoba and the Royal Observatory at Greenwich was made, and the results compared with the position of the observatory made under the direction of Dr. Benjamin A. Gould in 1871.[16] Every astronomer recognized Benjamin Gould as one of the most exacting and painstaking of observers and mathematicians. It would appear that he would have used every precaution to secure highest attainable degree of precision possible in his time. Dr. Gould based his position upon two very careful longitude determinations carried out by the U. S. Naval astronomical expeditions. These expeditions derived, independently, the longitude of Buenos Aires from Greenwich and that of Valparaiso from Washington. One of these determinations was made by way of the Atlantic and one by way of the Pacific. The results of the two determinations showed a satisfactory agreement within 0.05 seconds, or a linear uncertainty of some 16 meters.

In the determination of 1931, time signals were exchanged by radio in place of cable and the probable error from 35 nights' observa-

tions gave an uncertainty of the resulting longitude of but 0.007 seconds. When, however, the 1931 result was compared with that of 1871, the two determinations differed by 1.11 seconds, corresponding to a linear distance of 400 meters or 1200 feet,—an unthinkable difference to be charged to the account of observational errors.

In discussing the results, astronomer Zimmer, of the Cordoba Observatory, tacitly assuming that the observatory could not have moved, commented on these results as follows: "The remarkable thing about it is that two independent determinations such as those made by Green and Davis, should be in complete accord, and yet be wrong by a whole second. We have to assume that those two determinations were in error by at least a round second." Mr. Zimmer found it impossible to imagine any continental drift to allow for the change that had taken place, especially since he stated that, if one were to imagine any such movement, "the direction was opposite to that predicted by Wegener." One, perhaps, should be cautious in evaluating such a statement, lest it lead to a type of reasoning that may discourage discovery, by inhibiting the imagination, on the one hand, and erroneously assuming, on the other hand, that if a lateral movement had taken place, it would have to be in accord with some particular hypothesis. Perhaps, it is a pernicious imagination that would suggest that, if an orogenic movement had at one time been westward, there might be a reactionary period when a crustal shift could, for the time being, take place in the opposite direction. This is something for geologists to ponder over.

In closing this discussion of longitude variations, there remains one other observation to mention; namely, the results of a comparison of longitude determinations between San Diego and Washington, made during the campaigns of 1926 and 1933, and published in 1939.[17] A comparison of the observations of longitude reduction over this seven-year interval, taken at face value, showed that the longitude determination in 1933 was 0.045 seconds greater in 1933 than in 1926, an amount more than six times the probable error of the observations. This amount expressed in linear dimensions would correspond to an increase of 55 feet in the distance between the east and west coast. This discrepancy seemed so much larger than errors in the observations could allow, that the Naval Observatory astronomers re-examined the reductions of the 1926 observations and came to the conclusion that a small correction to the reading of the chronograph records by the earlier computers could be made so as to bring the results of the two series more

nearly in accord. Even after such corrections have been applied, it is found that, in 1933, San Diego appears 37 feet farther west than the 1926 observations indicated.

One should emphasize here that there has been a vast improvement in the technique of determining longitude in the last fifty years, largely through the advent of frequent time comparisons by radio. In place of being dependent upon cable lines, one can receive at will, in any field station, time signals broadcast with an accuracy undreamed of a century ago. Nevertheless, with every advance in obtaining another decimal place, one is sure to encounter new problems to be solved. In this case, we have no exception to the rule. In recent studies that we have made on the apparent elapsed time in the propagation of radio signals between distant points, it has been found that it is no longer safe to assume that the velocity of these radio waves is constant and equal to that of the velocity of light, an assumption all too easily made by those who have not faced directly some of the problems of communication by way of the ionosphere.

In a paper published in the Journal of Terrestrial Magnetism in 1936,[18] I showed that, in deriving the velocity of propagation of time signals employed in the longitude campaign of 1926, the apparent group velocity of the radio waves deduced on the assumption that the waves traveled the shortest possible path between stations varied in speed, all the way from values approximating the velocity of light to values approximating but a little more than two-thirds the velocity of light. Furthermore, there was indication that the speed appeared to depend more or less directly upon the intensity of the earth's magnetic field in the region over which the waves passed.

Even in the case of constant comparisons between two fixed stations, such as Greenwich and Washington, it was found that, over a period of six years, the velocity of the radio time signals could vary appreciably, for reasons not readily explained. The inference from this would seem to be that, in attempting to obtain a still higher degree of accuracy in determining longitude, we are involved in a study of ionospheric conditions, upon which the peculiarities of radio transmission must depend. For a higher degree of precision in longitude determinations, it would appear desirable that the observing stations involved should be in relatively close proximity to wireless stations which transmit the signals.

I now dismiss completely the matter of astronomical determina-

tions and call your attention for a moment to the possibility of measuring short periodical displacements of the earth's crust in the vertical direction. Thanks to the improvement in gravimetrical instruments within the last few years, it has been possible to measure variations in gravity with a degree of precision never before attained. While the modern gravimeter has been developed ostensibly for commercial purposes, one of these instruments placed for several months in a fixed location has revealed so unmistakably the rise and fall of the earth's crust in response to solar and lunar tides in the earth itself as to render the results unquestionable.

A few years ago, I took the opportunity of visiting the Gulf Research Laboratory in Pittsburgh, where, under the direction of Doctors Foot and Eckhardt, a sensitive gravimeter, set up on test in the basement of the laboratory for several months, had shown consistently both the solar and lunar tides in the solid earth.[19] The tidal curve reduced from the gravimeter observations, when compared with the calculated theoretical tide potentials, showed a definite phase lag of about 50 minutes between the time of the maximum tidal force and the maximum response of the earth's crust. While the experts in charge of this instrument felt that caution should be exercised in determining the amplitude of the curve, because of certain uncertainties in the calibration of the instrument, one could state with certain reservations that the range of movement of the earth's crust in the vertical direction during the lunar day represented a displacement of about 2 feet. Were there only this single incident, it is quite sufficient to emphasize the importance of the establishment of a number of such gravimeters in selected regions, to determine not only such differential displacements, but also to discover, if possible, regions where such displacements may exceed or fall short of the magnitude of the variations recorded at Pittsburgh.

As a possible cosmic cause for differential movements in the earth's crust, one can not forget the gravitational force exerted by the moon. It is always difficult to see how the very feeble stresses set up in the earth's crust by the lunar tidal attraction could have an appreciable effect in moving large continental masses. However, perhaps one should not forget the possible resultant effect through the continued application of intermittent stresses repeatedly applied in a given direction. Were it possible to think of extended regions of mobile matter within the earth, then perhaps tides set up in this underlying mobile

material could produce, through hydrostatic pressure, far-reaching effects in this connection.

A few years ago, I undertook a reinvestigation of the frequencies of the occurrence of major seismic disturbances, as possibly correlatable with the position of the moon.[20] While the results were not as conspicuously convincing as one might have wished, there was a definite indication that, at least, so far as deep-focus earthquakes are concerned, the curve of frequency deduced showed two maxima and two minima during the lunar day. Maximum values coincided with positions of the moon four hours and sixteen hours past the meridian. One might say that there are indications here of a trigger action, such that if, through accumulated stresses, large earthquakes are about to occur, there is a better chance of their occurrence during a time when the horizontal tidal component of the moon's gravitational force is at a maximum.

A few days ago, I received a communication from that venerable and esteemed geologist, Professor William Hobbs of Michigan, in which he said, "If you have published material proof of the changes of latitude and longitude, could you send reprints, or else references to place of publication?" By training and temperament, I think any scientist should be reluctant to use the word "proof," and, while the material I have presented may have fallen far short of what any mathematician could desire as proof, I hope I have fairly, and without undue prejudice, presented evidences that exist, in such a way as to encourage a degree of open-mindedness among both astronomers and geologists on so fundamental a question as that of differential movements now going on in the earth's crust. If so, then I shall have been successful, at least to the extent of having presented material consistent with the subject assigned.

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SECTION OF BIOLOGY

April 10, 1944

Professor W. M. Copenhaver, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *Effects of Liver Extirpation and Transplantation on Blood-Cell Formation.* (This lecture was illustrated by lantern slides.)

Since the liver is an organ with many functions, its removal produces a number of physiological disturbances. The present study has not attempted to cover all of the disturbances produced by the extirpation of this organ. The major emphasis has been given to the effects on the blood and blood-forming organs.

The present experiments on amphibian embryos were stimulated originally by the work on anemia in mammals, particularly by the work of Whipple and co-workers (1920, 1925) and Minot and Murphy (1926). Through their work and that of Castle and others, considerable information concerning the role of the liver in erythropoiesis has been gained. Castle has shown that the interaction of an extrinsic factor present in food and an intrinsic factor present in the normal gastric secretion leads to the production of an antipernicious anemia factor (erythrocyte maturation factor) which is stored in the liver, and, to some extent, in the kidneys. Pernicious anemia occurs when a lack of the gastric factor leads to a deficiency of the maturation factor. From the available evidence, we should probably expect an anemia after surgical removal of the liver, although, as pointed out by Castle (1936), the kidney is also a storehouse for the maturation factor.

A number of investigators have studied the effects of complete hepatectomy in mammals, but these studies have little bearing on the present problem because mammals usually do not survive the operation for more than 10 to 15 hours. This is too short a time for changes in the blood picture.

Adult amphibians survive hepatectomy longer than mammals, but it is doubtful whether they survive long enough to develop an anemia initiated primarily by the lack of an anti-anemic liver factor. The literature on this has been discussed more fully in an earlier report (Copenhaver, 1943).

Liver extirpation and transplantation in amphibian embryos was

studied by Holtfreter (1925), but his chief purpose was an experimental study of the development of the organ. He made no mention of anemia, but, from the present experiments, it is evident he did not keep his liverless animals long enough to obtain any effects on blood formation.

Amphibian embryos are particularly useful for the problem because the technique of hepatectomy is relatively simple (as compared with mammals) and the embryos survive for a considerable period. On the other hand, embryos have certain disadvantages, particularly their small size, which makes it difficult to obtain enough blood for good smear preparations.

NORMAL BLOOD DEVELOPMENT IN *AMBLYSTOMA*

As in mammals, several locations (mesoderm of yolk region, liver and spleen) are utilized for erythropoiesis at successive ages. After the spleen differentiates, it becomes the chief location for erythrocyte formation for the remainder of the life of the salamander, while a layer of tissue beneath the capsule of the liver functions for the development of granular leucocytes. In this respect, the salamander differs from mammals, where the succession of developmental stages leads to the formation of erythrocytes and granular leucocytes in bone marrow.

The morphological stages of hemopoiesis in salamanders have been illustrated clearly by Jordan and Speidel (1930). As in mammals, the earliest cell which contains a recognizable amount of hemoglobin is called a proerythroblast, and successive stages with more hemoglobin are called erythroblasts and normoblasts. The mature erythrocyte is a nucleated, large, oval-shaped cell which differs considerably from the smaller, non-nucleated human red corpuscle. The mature erythrocyte of the salamander is larger than the immature stages which precede it; in mammals, the reverse is true.

EXPERIMENTAL METHODS

Two species of salamander were used, *Amblystoma punctatum* and *Amblystoma tigrinum*. The majority of the operations were done on embryos at Harrison's stage 39, a stage 2 to 3 days after the beginning of circulation and only a few days after the initial swimming reactions. At this stage, the liver is relatively undifferentiated and consists of a pouch of yolk-laden endoderm attached to the floor of the gut.

The experiments fall into two main groups. In one series, the

liver anlage was removed and the embryos were reared as long as possible to study the effects of complete extirpation. In another series, the operation was done in two stages. First, a liver rudiment was transplanted from one embryo to the tail of another embryo and then, after a few days, the normal liver was removed from the host.

EFFECTS OF EMBRYONIC HEPATECTOMY

A total of 251 embryonic liver extirpations were done in this series, but some embryos failed to survive the immediate effects of operation, and others were fixed for histological study at different ages after operation. One hundred seventy-nine animals were kept until they died, before being fixed for histological study. Some of these were reared under normal conditions, others were fed on liver, and still others were given injections of liver extracts. The extracts were supplied by Dr. Randolph West and were known to be effective in the treatment of human pernicious anemia. There was no significant difference between the survival periods of the untreated animals and those given liver extracts. The maximum survival for untreated *Amblystoma tigrinum* was 41 days after operation (33 days after the yolk resorption stage), with a mean of 20.5.

All animals which survived much beyond the yolk resorption stage became very anemic. It is interesting that anemia never developed before yolk resorption, but feeding the animals on yolk of younger embryos did not prevent it. The anemia was observed first by studying the gill circulation of living animals and was checked later by serial sections. In extreme cases, there was complete absence of mature erythrocytes and only a few erythroblasts.

There was no liver parenchyma, but the ventral mesentery of the liver region contained developing leucocytes similar to normal subcapsular hemopoietic tissue. The differentiation of this tissue is apparently no dependent on the development of the liver.

The spleen was hypoplastic and its vessels were usually empty.

LIVER EXTIRPATION COMBINED WITH TRANSPLANTATION TO AN ABNORMAL POSITION

In this series, the animals had no liver tissue except the graft. Although they did not grow as rapidly as normal animals, they survived for a long period. Some were reared beyond metamorphosis. They showed no signs of anemia.

Sections through the transplants showed parenchyma cells arranged in cords between well-developed sinusoids. Bile canaliculi and bile ducts were present, but they did not connect with the gut.

The grafts had vascular connections with the caudal artery (dorsal aorta of tail) and with the caudal vein. Although the main blood supply was arterial, in contrast to the normal condition in which the major supply is from the portal vein, the parenchyma cells stored a considerable amount of glycogen.

In a number of cases, the grafted liver was removed after the animals had been kept for a period ranging from 65 to 132 days. The maximum post-operative survival was 8 days. These older animals died with symptoms of liver deficiency similar to those described by Magath and Mann (1925) for dehepatized frogs. They did not become anemic; the survival period was probably too short for changes in the blood picture.

DISCUSSION

In attempting to explain how embryonic liver extirpation causes anemia, a number of factors have to be considered. Whipple (1922) and Robscheit-Robbins and Whipple (1925) have concluded, from their studies of anemia in dogs, that liver produces a parent pigment complex which is essential for the formation of hemoglobin. Animals without a liver would be expected to lack the parent pigment substance necessary for hemoglobin formation and they would also be expected to have a deficient amount of the erythrocyte maturation factor. Lack of pigment should reduce the quantity of hemoglobin and the size of the erythrocytes, whereas the lack of the maturation factor should produce larger cells or macrocytes as in pernicious anemia. It should be possible to check these points, but in most of our experimental animals there were very few erythrocytes left when the animals were fixed for sectioning. In most cases, the animals passed from an approximately normal to an extremely anemic condition within a relatively short time. From a study of serial sections, the few erythrocytes present in the final stages did not appear abnormally large.

Another difference between the anemia produced by embryonic hepatectomy and human pernicious anemia is that, in the former, the blood-forming organ (the spleen) is very hypoplastic, whereas, in the latter, the blood-forming organ (the bone marrow) is hyperplastic with many immature cells.

It might be supposed that the experimental anemia is produced by removal of a blood-forming organ. The liver is normally an organ for erythrocyte formation for a period of embryonic development, but since the anemia does not become pronounced before the spleen normally begins to function in erythropoiesis, the results do not prove that the liver is necessary as a locus of blood formation. The decreased size of the spleen is probably not a primary cause, since Jordan and Speidel (1930) have shown that splenectomy in adult *Triturus* is followed by compensatory erythropoiesis in the general circulation. The lack of erythrocyte proliferation, either in the small spleen, or in the general circulation, appears to result from the absence of some substance normally produced in the liver.

The fact that all liver extract injections have thus far been ineffective indicates that the cause of the anemia is not simply the lack of storage of the erythrocyte maturation factor. Functioning liver cells, such as were present in heterotopic grafts, seem to be necessary.

In summary, it may be stated that amphibian embryos survive complete liver extirpation for a period which is sufficiently long to produce anemia. The anemia does not occur during the embryonic yolk stages, but begins about 1 week after yolk resorption. In the final stages, from 20 to 40 days after operation, there are few or no erythrocytes left in circulation. The anemia has not been altered by injections of liver extracts but can be prevented by transplantation of liver tissues to other parts of the body.

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SECTION OF PSYCHOLOGY

April 17, 1944

Doctor Eugenia Hanfmann, Assistant Professor of Psychology, Mount Holyoke College, South Hadley, Mass.: *Approaches to the Intellectual Aspects of Personality.*

The majority of psychologists, today, agree that personality should be conceived of as a patterned whole, all parts of which are interrelated. Yet, this recognition of the structural relatedness of all aspects of personality has influenced but little the psychologists' treatment of the intellectual functions of the individual. MacKinnon, in his recent survey of the theories of personality, describes the situation as follows: "Character and temperament have loomed large in all discussions of personality, but not intelligence. . . . It is a neglect which cannot be justified, for certainly intelligence is as integral a part of the personality as are character and temperament."

Whatever the reasons for this neglect of intellectual functions as a personality aspect, a re-orientation must take place in this field. The problems that must be considered are not new to psychology as such. The existence of qualitatively different patterns of intellectual performance has been recognized and these patterns have been extensively investigated in the field closely relatd to the psychology of personality, namely, in developmental psychology. This term is used here in the wide meaning given to it by Heinz Werner, namely, as the study of development of mental life in an individual, or in a social unit, or in an animal species. Taken in this wide meaning, developmental psychology includes also the study of regressive phenomena found in the field of psychopathology. Since my own work on intellectual processes has been largely in the abnormal field, I shall briefly review some of the experimental and clinical studies carried out with the schizophrenic and organic cases, and then I shall try to show how similar methods of investigation could be applied to the problem of the personal patterns of intellectual performance in normal subjects.

If one is studying intellectual processes with the view of discovering qualitative differences rather than simply measuring achievement, one must present the subjects with tasks that permit a wide variety of solutions, or of methods of solution. One such task we found in the

picture completion test devised by Healy. The subject is presented with a series of pictures from each of which a square piece has been cut out, and is asked to choose from a number of small picture blocks the one that best completes the sense of each picture. Upon giving this test to a group of patients of a state hospital and to a group of normal controls, we found that two-thirds of the patients with organic brain pathology and one-third of the schizophrenic patients showed incongruous or absurd placements which were practically absent in the normal group. These peculiar placements were of different types. Sometimes the patient completed only the represented action, without any regard for the rest of the situation. Thus, in the picture representing children in the fields who are looking up at something, the subject may place in the portion of the picture representing the sky, a man, an automobile, or a dog, commenting simply: "Those boys are looking up at a dog." Or he may go even farther in his disregard of the physical reality and not refer at all to the outward action represented in the picture, such as looking at, or pointing to an object. Instead, he bases his placement on psychological moments, such as the thoughts, needs and wishes of the person represented. The picture of a tie, for instance, is placed on the horizon with the comment: "The boy needs a tie for his collar," or a horse is in the room because "the boy wants a horse—he wants to become a traveller." Finally, even the specific connection of wishing or thinking, obtaining between the person and the object, may be dissolved in favor of a more indefinite impression that a certain object belongs within the sphere of the represented situation or activity. Thus, a milk bottle may be placed on the wall because "people are having breakfast."

Occasionally the patient's wishes or fears influence the completion. One patient, for instance, decides against using the block with the picture of spilt ink, because, as he says, he himself would not want to drop the ink. Or he may protest that he is not the one who smashed the face of the boy in the picture. The picture, in such cases, seems to be given the status of reality.

Completions colored with personal reference were more common in the schizophrenic than in the organic group. The same was true of another kind of peculiar completions, which might be described as symbolic summaries of the pictured situations. The object inserted does not belong to the same concrete sphere of action as other objects in the picture: rather, it summarizes the pictured scene, giving its meaning,

or its keynote. Thus, pictures of books are used as providing a key to school scenes: "The book is up—that means the boy is going to school"; a broken flower pot is inserted into the picture representing an accident, because "the boy's head—his mind—is broken, as is the flower pot"; clocks are made to stand for the events that usually fall into a definite time of the day: "six thirty—time to go to supper."

All peculiar completions have, in common, the disregard of spatial relationships. The inserted objects are placed in positions that are impossible within the spatial system represented: the room, the street, the field. It is this disregard of the physical impossibility of certain positions that makes most of the completions described appear incongruous or absurd to a normal observer. The questioning of the patients brought out clearly that they were well able to grasp the represented environment; e.g., in the picture where the boys were made to look up at a clock, they were understood to be playing in the field, with no object to which a clock could be affixed. Yet, even though the patients, in response to the examiner's urging, might occasionally admit that the position is strange, they showed no concern about it, and no desire to change the placement. From the comments of the patients we were forced to conclude that they were not unable to appreciate the spatial details, but that they simply denied the inserted object a position in space and, in general, denied the realistic character of the pictorial representation which a normal person in this situation usually takes for granted. The spatial relationships do not constitute for the patients a stable frame of reference which for normal subjects determines and limits the possibilities for various positions and sizes of objects. This lack of a spatial frame of reference may be considered as an example of general ineffectiveness of all stable frames of reference in schizophrenic thinking.

A second approach to the study of the schizophrenic thinking was made with the help of a sorting test devised by Ach and modified by the Russian psychologist Vigotsky. The test material consists of 22 wooden blocks varying in color, shape, height and size. On the under side of each block a nonsense syllable is written. Regardless of color and shape, *lag* is written on all tall large figures, *bik* on all flat large figures, *mur* on the tall small ones, and *cev* on the flat small ones. The subject has the task to find and separate these four kinds of blocks. After each incorrect attempt at grouping, the examiner turns up one of

the wrongly selected blocks and encourages the subject to continue trying until the required solution is found.

The test may be scored quantitatively, in terms of the amount of time and help required for a solution. Its main value, however, lies in the possibility of a very detailed qualitative analysis of the procedure of the subject, whose manipulations of the blocks reflect nearly every step in his reasoning.

Normal subjects of high educational level see the task clearly as one of classification and try out different possibilities in succession: color, shape, size, etc. The groups of blocks they form in this process represent classes, all members of which have one characteristic in common. An extreme deviation from this logical procedure is shown by those patients who form groups based on similarities between individual blocks. To a *small red* triangle the patient may add a *small red circle,* next to this is placed a *large* blue *circle,* next to this a *large* square, so that the resulting chain of blocks has no common denominator. In producing such and similar complexes of blocks the patients, instead of formulating and testing hypotheses, seem to be passively led by the material at hand. Even when, with the examiner's help, the correct grouping has been established, the patient is unable to see the principle on which it is based and to reconstruct the groups after they have been destroyed. In this, the patients differ from the normal subjects of lower educational level, who frequently start grouping blocks in a rather primitive fashion, but are always able in the end to understand the basis of the correct solution. From one-third to one-half of the schizophrenic patients show definite impairment of conceptual thinking on this test, and the patients with brain pathology perform, as a group, on a still lower level.

Let us consider now, in more detail, the performance of the normal subjects of high educational level. Although all of them performed on the highest conceptual level, they differed typically in their approach to the task. One group preferred to formulate their hypotheses in advance, paying minimum attention to the blocks in front of them and using them only for the testing of their hypotheses. Frequently, these subjects even refused to touch the blocks until the classification was quite clear in their minds. The other group of subjects was quite absorbed in the material, got ideas for grouping in the course of the process of grouping itself, and frequently achieved the correct classification before having clearly formulated the conceptual basis for it.

The first type of approach was more frequent among men, the second among women. Regardless of whether the conceptual or the perceptual approach predominated in a given case, some subjects showed a good integration of these two aspects of performance, whereas others consciously fought the intrusion of perceptual tendencies, thereby lowering considerably the efficiency of their performance. We may assume that these different patterns of intellectual performance bear a significant relationship to other personality aspects, a relationship which should be further investigated.

The relation of intellectual to other aspects of personality may be seen clearly in abnormal cases showing an extreme change in intellectual processes. One such case, soon to be published, was investigated by K. Goldstein, M. Rickers-Ovsiankina, and myself. The case was one of brain pathology produced by injury to the head. The disturbance of cognitive functions was extreme, showing itself even at the level of perception. The patient was unable to recognize isolated objects, regardless of the mode of their presentation. In his attempts at naming them he would be led by different clues in succession, but would never achieve correct recognition. Yet, if he could successfully perform an action with an object—e.g., write with a pencil, unlock a door with a key, throw a ball and see it bounce—he would recognize and name the object immediately and securely. Because of this mode of recognition, objects offered in a setting permitting action (e.g., key with lock) were recognized better than those presented in isolation, and pictured objects that could not reveal their nature through real action were recognized worst of all. Application of definite categories which insure structure and relative constancy in the case of normal perception was missing from the perception of the patient. An object for him was only an integral part of an action, and had little meaning outside of this context.

The use of language was another function that reflected the drastic changes in the patient's intellectual processes. He was able to understand and use language only in connection with actual concrete situations. When being told a simple story he would look around for people and objects mentioned in it, and, not finding them, was unable to apprehend the meaning of what he heard. The use of verbal symbols as a means of operating at will, with non-present or even non-existing things, was closed to the patient.

The memory function was similarly disturbed. A few minutes

after some episode had taken place, the patient was completely unable to recall it, if the situation had changed in the meantime and he was engaged in a different activity. Isolated questions directed at him were never sufficient to produce recall. Only if some aspect in the present situation vividly reminded the patient of a former one, was he able to recall it. In this case he re-enacted the whole scene which, for the moment, seemed to become reality for him. Yet, even his successful recall differed from the normal one, in that he was unable to localize the past event in time and to relate it to other events of his life, past or present. The memory of the patient provides him only with a number of suddenly emerging concrete episodes, but not with a continuous picture of his past. His whole experience totally lacks time-perspective.

How do these deep-going changes of cognitive functions influence the patient's emotional, evaluative responses to his environment? First, the patient responds emotionally only to the situations he is able to grasp, which limits considerably the scope of his response. On the other hand, he responds most intensively to the events he can and does grasp: thus, he always praised excessively all the objects that functioned adequately or fulfilled some useful purpose. In social situations the patient responded strongly and adequately to friendly approaches, to offers of help or of presents. He could always be relied upon to give away any of his belongings in response to somebody praising them. Equally strong negative emotions were occasionally aroused by situations of strong frustration. At first glance, these emotional responses seem to be understandable and essentially normal. Actually, however, they show many unusual aspects. The very strength of emotions is due to the abnormal narrowing down of the mind to the one momentary impression. The same close dependence of the concrete situation results in another deviation from the norm, which has a far reaching significance for the patient's relations to other people. Since people do not exist for him when they are not present and active, he has no permanent attitudes toward them and responds only to their momentary actions. Consequently, he never initiates any social actions himself, and no permanent personal bonds are formed between him and other people. Even within his own family, the patient remains essentially a stranger.

This brief description of a case of an extreme change in cognitive functions illustrates how the loss of cognitive frames of reference leads to deep-going changes in the total behavior of the patient and in his

emotional attitudes. The connection is such a close one, that it seems hardly possible to isolate the cognitive functions as such from the rest of personality. Goldstein, therefore, is reluctant to speak of the disturbance of the cognitive functions as such, and prefers to speak of the loss of categorical behavior, which implies a change in all fields of experience and performance. It is to be hoped that, by applying a similar type of analysis to the investigation of qualitative variations within the range of normal intellectual performances, we shall in time be able to see these variations as reflecting significant differences in the underlying personality patterns.

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Doctor Ernst Kris, New School for Social Research, New York, N. Y.: *Art and Regression.**

In this paper, I deal with the art of the insane. Before entering into my subject, I should like to comment upon the terms used in the title: In referring to art, I do not refer to activities that are in the nature of art, such as drawings of children or of the unskilled, but a social phenomenon which presupposes a certain level of performance. The term regression stems from neurology and acquired psychological significance through Freud. He distinguished various types of regression. He speaks of (libidinal or) historical regression if an individual returns to an earlier stage of his own development; that is, if the child of four, faced with deprivations, turns temporarily to thumbsucking. Historical regression occurs as incident in the limits of normal development. No psychopathological phenomena are known without some kind of historical regression.[1] Freud speaks of Ego regression or (topological regression) in order to indicate that the Ego has "renounced" some of its functions.[2] The functions of the Ego during sleep are "reduced"; a lower level of control prevails than in the waking state. However, the Ego has not ceased to exercise its influence. The chaos of the dream is an example of regular, temporary and partial Ego regression. The secondary elaboration of the dream, the attempt to adapt chaos to an apparent order, is the best known vestige on continued control. In the context tonight, both temporary and permanent regression of Ego functions will be discussed. We shall ask ourselves what kind of "temporary" regression occurs wherever art emerges, and what happens to art if, in a state of "permanent" regression, in insanity, the artist continues to produce art.

I have chosen so pointed a formulation, not because it is one which throws light on the dark problem of what is beauty and why man enjoys it, but rather in order to investigate a specific problem. Psychology of art is a dormant science, if a science at all. It therefore seems permissible to apply an old device of research: The study of "Mental Pathology," of "The Experiments of Nature" (Dilthey, 1886), has led

* This address was delivered at the meeting of the Section of Psychology, March 20, 1944.

[1] I will not here discuss the problems of historical regression any further, as I do not propose to deal with it. For definitions, see Freud's "Meta-psychological supplement to the theory of dreams," [1916], Collected Papers, 4. For a discussion of historical regression, see K. Lewin, "Frustration and regression," University of Iowa Studies, 18, No. 1, 1941. For other attempts at definitions, see Sears, R., "Survey of objective studies of psychoanalytic concepts," Social Science Research Council, Bulletin, 51, 1943.

[2] Ego regression also implies a genetic concept, one referring to stages of Ego development. Ego, in Freud's definition, is not the "self," nor the "I," but a system of biological control functions.

to an understanding of many psychological processes in the normal. We may try for once to see what we can learn for an understanding of art from the products of some individuals who, known as artists, continued to work in the darkness of psychoses. However, I should like first to dispel an apprehension: I do not aim at any general theory which would once more try to link all art to insanity, nor do I recommend the creative genius in general as a subject for pathological investigation. In limiting my discussion to that of Ego functions, or rather of one Ego function, I hope to throw some light on the relationship between art and insanity, which has proved so inspiring and misleading at the same time.

The problem was formulated first by Plato, in "Phaedros" and "Ion." Art, he says, is produced in a state of creative madness, in a "divine release from the ordinary ways of man," a state of trance or ecstasy which in many ways is similar to "madness produced by human infirmity." In psychoanalytic terms, this might be formulated as follows: In creating art, the artist is in a state in which the control of the Ego is reduced. A regression takes place, but the regression is temporary only, and the Ego uses it for its own purposes. In certain psychotic states, especially in schizophrenia, the regression reduces the ability of the Ego more permanently and more completely; the loss of function is (in the ideal case) irreversible as long as the condition inheres. The Ego does not "use" regression by regaining control over it but (at least frequently) the Ego is overwhelmed by regression.[3]

The first part of this formulation refers to inspiration.[4] Not all artistic creation derives from inspiration; neither all kinds nor one kind wholly. But, wherever art reaches a certain level, inspiration is at work.[5]

The phenomenology of the process of inspiration, the experience of the artist during creation, is well known from many self-observations related through the ages. Wherever men live, some similar process occurs when they create; though what they produce is not

[3] For a more detailed theoretical formulation of the relation of Ego control and regression, see the following papers: Kris, E., "Psychology of caricature," International Journal of Psychoanalysis, **17**, 1936, part 3; "Ego development of the comic," *ibid.*, **19**, 1937; and Hartmann, H., 'Psychoanalysis and the concept of health," International Journal of Psychoanalysis, **20**, 1939, pp. 308 ff.

[4] Kris, E., "On Inspiration," International Journal of Psychoanalysis, **20**, 1939, pp. 377 ff, and Harding, R. E., "An anatomy of inspiration," Cambridge (England), 1940, who discusses the "inspirational nucleus" of artistic creation.

[5] This may seem a transgression from psychology to normative thought, i.e., to "aesthetics" in the traditional sense. I am not so sure that it is a transgression, but if so, it is one I intend. I believe that the most sensible, and in fact most frequently applied, evaluation of art, by contemporaries of the artists or by posterity at subsequent periods of time, refers, consciously or unconsciously, less often to skill or perfection than to depth of meaning and expression, which inspiration alone seems to convey.

always in the nature of art. All self-observations assert the absence of conscious volition: *It* works, and not the man. "I have two or three times in my life composed from the wish rather than from the impulse," says *Burns,* "but I have never succeeded." To *Wordsworth,* poetry is a "spontaneous overflow of powerful feelings." *E. A. Housman,* whom I quote because what he says tallies closely with impressions gained by psychoanalytic observation of several individuals active in the arts, has put it in these terms:

"In short I think that the production of poetry, in its first stage, is less an active than a passive and involuntary process; and if I were obliged, not to define poetry, but to name the class of things to which it belongs, I should call it a secretion; whether a natural secretion, like turpentine in the fir, or a morbid secretion, like the pearl in the oyster. I think that my own case, though I may not deal with the material so cleverly as the oyster does, is the latter; because I have seldom written poetry unless I was rather out of health, and the experience, though pleasurable, was generally agitating and exhausting. If only that you may know what to avoid, I will give some account of the process.

"Having drunk a pint of beer at luncheon—beer is a sedative to the brain, and my afternoons are the least intellectual portion of my life—I would go out for a walk of two or three hours. As I went along, thinking of nothing in particular, only looking at things around me and following the progress of the seasons, there would flow into my mind, with sudden and unaccountable emotion, sometimes a line or two of verse, sometimes a whole stanza at once, accompanied, not preceded, by a vague notion of the poem which they were destined to form part of. Then there would usually be a lull of an hour or so, then perhaps the spring would bubble up again. I say bubble up, because, so far as I could make out, the source of the suggestions thus proffered to the brain was an abyss which I have already had occasion to mention, the pit of the stomach. When I got home, I wrote them down, leaving gaps, and hoping that further inspiration might be forthcoming another day. Sometimes it was, if I took my walks in a receptive and expectant frame of mind; but sometimes the poem had to be taken in hand and completed by the brain, which was apt to be a matter of trouble and anxiety, involving trial and disappointment, and sometimes ending in failure."[6]

The passivity of the experience of inspiration may be rationalized in various ways: Not the artist creates, but a voice from above. He is not responsible as a person, but the genius to whom he submits is responsible. If we look at the process, as we sometimes are able to do, with the psychoanalytic microscope, we find that an impulse suggested by an instinctual demand or drive reaches, in this state, the preconscious level ("with unaccountable emotion") and is elaborated without the awareness of the creator. The form then appears ready and complete before his mind. In all creative thinking, similar processes can be observed. Neither in science nor in statesmanship is thinking entrusted to awareness only; preconscious elaboration is part

[6] Housman, A. E., "The name and nature of poetry," New York: The MacMillan Co.; Cambridge, England: At the University Press, 1936, pp. 47-49.

of almost all thought. But in art it is different. There always exists an immediate link between preconscious thought and basic needs or repressed memories—the Id, in Freud's definition.[7] Were we to represent this process in a model, we should say that the psychic system is to some extent open to the unconscious; that an Id impulse is elaborated on a preconscious level and this elaboration is then sometimes consciously, and with pain and struggle, further refined.

The experience of inspiration reaches from trance to slight drowsiness, or simple immersion in work. But, however intensive the preoccupation of the artist, it is, as a rule, limited to the process of creation. When the work is done, the painter looks at his canvas, the novelist rereads his script, the poet his poem, and the musician listens to the first accords. A shift has taken place—the creator has become his first audience. And now, in him as in every audience, the process of creation is repeated. As one is gradually drawn into the work of art, one relives on a different level what the artist has said. The way leads from the conscious to the preconscious, and only then is unconscious material set in motion.[8]

Every work of art establishes a link between the artist and a public. Art, in society, serves various purposes. It may be moralizing or it may be inciting. Hardly ever is it quite free of such a function; but, at no time, is great art only subservient. The artist may address one other fellow-man, or he may be conscious of striving for the admiration of many. He may, like artists in some primitive societies, address a community of fellow-experts, who, with some sign of deference, bow to his genius. Or he may address the unstratified audience of mass society. *But art, at all times, is communication from one to some others.*

In clinical observation of those who create, we find that the idea of a public is ever present. The shift in narcissistic cathexis, from the self to the product, accounts for the importance of this relationship. The work of art is to the artist frequently—and during creation always—part of the self; and when, after its completion, he looks upon it, himself part of the public, he still is closer to what he has produced than

[7] The work of art may give a modified expression to these Id impulses, or they may be perceived as dangers; the work of art then serves as a defense. Mostly, it serves both functions at the same time.

[8] Kris, E., "Approaches to art," in S. Lorand, "Psychoanalysis today." The International University Press, New York, 1944.

all of the others. Their approval of his work is, in fact, the essential approval of his own self.[9]

I shall here attempt to summarize these introductory reflections: Inspiration in artistic creation is a state in which the Ego has loosened its control; in which thoughts and images drive to consciousness and are formulated in shapes and signs "understandable" to others. Their understanding need not be correct. Art is as frequently and as regularly understood as misunderstood, and it is the secret of artistic formulation that it leaves the way open to a great many possible interpretations. The state of elation which art conveys to the public is lastly due to the fact that the play of emotions, the shift in cathexis between the psychic systems, repeats a process similar in nature to that which the artist experienced in creation. In "recreation," one might say, we repeat the process of creation in reverse.

I now turn to clinical examples. Most authors who have studied works of art produced by the insane, Prinzhorn in Germany, Bertschinger, and others, in Switzerland, Marie and Vinchon in France, and Nolan Lewis in the United States, have stressed the similarity between insane and primitive figurative expression.[10] This similarity which goes very far indeed, so that occasionally sculptures produced in institutions in urbanized areas closely resemble carvings from Oceania. Examples of this kind are highly impressive. The general explanation of these phenomena follows from some of Freud's basic assumptions which I formulate as follows: Differentiation of psychological structure is the work of the Ego; the Id is undifferentiated; it is not subject to cultural differences or, more correctly, it is subject to them to a much lesser degree. When, in regression, the Id comes to the fore, uniformity prevails.[11]

The problem I intend to discuss is less general. I shall deal with changes in the work of three artists during psychosis. I choose artists rather than laymen, since "the art of the unskilled" opens up problems

[9] This brief summary does not take into account various, not infrequent, deviations: indifference or abhorrence of the artist in front of his work is by no means rare. We have reason to believe that such attitudes are related to the unconscious determinants of production.

[10] Prinzhorn, H., "Bildnerei der Geisteskranken," Berlin, 1922; Bertschinger, H., "Illustrierte Halluzinationen," Jb. f. psychoanalytische und psychopathologische Forschung, 1911, **3**, 69-100; Marie, A., "L'art et la folie" Rev. Sci. Paris, 1929, **67**, 393-398, "L'expression artistique chez les alienes," Arch. Intern. neurol., 1931, **50**, 211-212; Vinchon, J., "L'art et la folie," Paris, 1924; Lewis, Nolan, D. C., "Graphic art production in schizophrenia," Proc. Ass. Res. Nerv. Ment. Dis. for 1925, 1923, **4**, 303-308.

[11] The archaism in Ego-regression of certain psychoses is, since Storch's investigations, a well-known phenomenon. Storch A., "The primitive archaic forms of inner experience and thought in schizophrenia" (German, 1923), Nervous and Mental Disease Monographs, No. 36.

of a specific kind.[12] I choose artists who suffered from paranoid schizophrenia, since, contrary to the opinion of Prinzhorn and others, I assume that the nature of the psychotic process is of considerable influence on the type of change in creative behavior.

In limiting the problem to one psychosis where the psychobiological process of regression is known from a wealth of clinical data, we hope to gain ground for a discussion of the transformation which Ego functions undergo during the process.

Our material does not lend itself to a study of the time perspective of the process. The urge to create sometimes arises in prodromal or early stages of schizophrenia. The urge then resembles, occasionally, that which we are wont to find with some adolescents. In several cases of poets it is possible to study the gradual transformation of style, as Jaspers did in the case of Hoelderlin[13]; or as it might well be done with the poetry of John Clare (1793–1864), whose unforgettable stanza, "I AM," reveals a striking analogy to Hoelderlin's work. In the poetry of both, the attempt to regain contact with the world and the self produces a gradual change in style, which leads Hoelderlin to new fervor in expression, and gives to the poetry of John Clare a greatness never accessible to the peasant poet who, in 1820, had gained recognition by his tame and polite versification. Problems of such gradual transformation must here be excluded, because the data available do not permit inferences of this kind.

The first example is chosen for the purpose of introduction. The Swedish painter, Ernst Josephson, had a psychotic break in 1898, at the age of 37. In his youth he had been the leader of the younger generation of Swedish artists. At about 30, he was recognized in Paris, where his paintings were shown in the "Salon." For 18 years, during insanity, he continued prolific activity as a draftsman. The conventionality of his earlier pictures disappeared. Whatever he drew in insanity seemed to be of greater boldness. It is as if he were no longer bound by the conventions of his era.[14]

On the other hand, we find that no clear insight can be gained into the meaning of his work. If we compare a nude, painted for the

[12] I have outlined these problems on a previous occasion ("Bemerkungen zur Bildnerei der Geisteskranken," Imago, 1936, **22**, pp. 339-370). Lately some of my hypotheses were confirmed by Anastasi, A., & J. P. Foley, "An analysis of spontaneous artistic production by the abnormal," Jour. of Genetic Psychology, 1943, **28**, pp. 297-313.

[13] Jaspers, H., "Hoelderlin und Van Gogh," Berlin, 1922.

[14] Similar changes are not infrequent. See: Anastasi, A. & J. P. Foley, "A survey of the literature on artistic behavior in the abnormal," Psychological Monographs, **52**, No. 6, 1940, pp. 23-28, 38-39, where the relevant literature is surveyed.

fashionable public of his time in Paris, with a drawing representing David and Goliath produced in insanity, the difference is obvious. In every detail, the working of the primary process stigmatizes the product of the insane. Goliath has three eyes, as have the giants of myths. A mask replaces, on the armor, the protrusion concealing the genitals. In another drawing, God, creating Adam, looks upon the world out of eyes formed by straight lines radiating from a center, with an expression more intense than anything that Josephson had previously produced. Out of God's mouth, breath is infused in straight lines to Adam's mouth. This contact from mouth to mouth has an almost sexual character. But then there are lines in these and in other drawings in which meaning is gone and stereotypic repetition is dominant.

Josephson's work had considerable influence upon a later time. The work of his insane period was rediscovered in the 1920's, and was then re-interpreted by "expressionists."[15] The detailed psychiatric data concerning Ernst Josephson have not yet been published, to my knowledge, and so this first case remains fragmentary.

The second case leads one step farther. There are more data available, though the artist, F. X. Messerschmitt (1736–1783), lived more than one hundred years before Josephson.[16] He was an Austrian baroque sculptor of renown. In his early days, in the 1760's and '70's, he enjoyed the admiration of the courts of Bavaria and Austria. Already, in the 1760's, he had several attacks of paranoia schizophrenia with persecutory ideas; and, in the 1770's, he finally had to withdraw from office. He had been a teacher of sculpture at the Imperial Academy of Fine Arts in Vienna, and his colleagues there had detected signs of insanity. The medical profession concurred with their judgment and petitions were sent to the court in order to guarantee a pension to a disabled man of great merit. In insanity, he withdrew to a little town some 50 miles outside the capital, into Hungary, and there, in 1781, he was visited by one Frederick Nicolai, a philosopher of the German Enlightenment and a bitter opponent of all Romantic thought; so bitter, indeed, that he violently attacked what seemed to him bombastic aspirations of Schiller and Goethe.

The enlightenment of the 1780's had, in various ways, opened up

[15] Kris, E., "Bemerkungen zur Bildnerei der Geisteskranken, Imago," 1936, **22**, pp. 339-370 (with illustrations).

[16] Kris, E., "Die Charakterkoepfe des F. X. Messerschmitt, Versuch einer historischen und psychologischen Deutung," Jahrbuch der Kunsthistorischen Sammlungen in Wien, 1932, pp. 169-228; "Ein geisteskranker Bildhauer," Imago, **19**, 1933, pp. 384-411 (with illustrations).

interest in psychiatric problems in Germany, and a magazine was published dealing largely with psychopathology.[17] Nicolai came from this circle. He interviewed the sculptor, Messerschmitt, for several hours, and in a book of travels—an "Inside Europe," as we might call it, of his day,—he reported the conversation. It gives us a clear picture of Messerschmitt's delusion.

Before I refer to it, I should like to discuss briefly the group of works with which Messerschmitt, a famous portraitist, was mainly concerned during his psychosis. It was of a scientific nature and dealt with a study of human physiognomy. He was working on a set of bronze busts which should represent the facial expression of man in various stages of emotion. This task was, at the time, entirely new. Artists who, in the great academies, had studied physiognomy before had chosen a different approach. For each of the emotions they had suggested one type of head and had endeavored to find what type,—girl or matron, adolescent or mature man—was best fitted to represent rage, adoration, joy, or pain. In the 1740's, however, in England, anatomists had started out to study the problem of how *one human face reacts to the change of emotions*, thus indicating an empirical problem. Messerschmitt followed this lead, and, in the first bust of his series, he represented one face, sleeping, yawning, and smiling. The face was always his own. While he was developing more of his physiognomic studies, his delusion got hold of him. He suddenly was convinced that he had discovered secrets of nature, long hidden by the Gods; therefore, evil demons were persecuting him. The persecution was both of an anal and an oral nature; thus, the mouths of all his busts (there are more than forty of them) are closed, so that nothing can enter. He rationalized the closing of the mouth in saying that the red of the lips was an invitation to sexual excitement. It was a mistake of nature that men should have lips, since animals had none.

This jump is characteristic of schizophrenic thought. The relationship between the human and the animal head had been a much discussed topic among artists, and a long heritage going back to Aristotelian tradition was kept alive in the academies of art. Messerschmitt knew treatises on physiognomics which were based on comparisons between human and animal features. The defense against the intruder through the mouth, suggested by his delusion and this lit-

[17] "Magazin fuer Erfahrungsseelenkunde als ein Lesebuch fuer Gelehrte und Ungelehrte." 1780 ff.

erary tradition, were condensed as ideas are regularly condensed by the primary process. Messerschmitt varied the rendering of the lips. In order to close them he sometimes put a band over them. At other times, as if in spite, the lip is made to protrude; and, in two cases, in heads which he refused to look at when he was visited by his interviewer, the lips are pulled into long, extended shape, which, as Messerschitt said, was meant to frighten the spirits. Here, clearly, the oral intrusion of the persecutor is depicted, and, we shall add, depicted with some greatness (Fellatio phantasy).

Before I comment further upon this case, I shall add what I know of the reaction of the public to the insane artist. Famous as he was, he could not be ignored, and shortly after his death, the rewriting of history began: he had never been insane, but a genius; he had left his teaching position, not because he could no longer teach and was a danger to his pupils, as the well-meaning professors had said, but, said the historians, because the professors were jealous of his greatness. If one takes the trouble to unravel the vicissitudes of local historiography, one finds that by the middle of the 19th century, some seventy years after his death, the understanding of Messerschmitt's colleagues and benefactors had been obliterated and, instead, the paranoid interpretation of the insane had been accepted as historical truth. He was set up as a genius unrecognized by his contemporaries. Needless to say, there is no foundation for this myth. It took me, in more leisurely times, several years to piece together the evidence concerning these distortions and the reaction of the public to the busts themselves. Over and over, it was attempted to interpret their meanings in terms of physiognomy: here was a laughing man, here a man in defecation. Such interpretations sometimes hold good. There is something of this or that expression to be found. But few people ever had the opportunity of looking at the whole series of busts. In order to ascertain the reaction to the whole series I conducted several experiments. If faced with all of the busts, subjects would continue to give names, i.e., "interpret" two, three, four, or five—up to eight. After that, they would lose interest, recognize something stereotypic, and end up with the discovery that, in spite of changes of hairdress and of shape, it was always the same head and always the same type of expression.

Let me summarize: Here is an artist whose insanity breaks into a clearly outlined program of work. In insanity, his work suddenly serves two new purposes: the first is linked to the original goal, but

while he sets out to study the human face in its changing expressions, he ends up by standing before a mirror and changing his hairdress and his features in search of a natural expression. The clinic of schizophrenia knows similar phenomena. I am inclined to consider this as a process of restitution. In search for what no longer can be synthetized, substitutes are adopted—substitute expressions instead of real expressions, or grimaces instead of natural physiognomies.

The second purpose which the work is made to serve is even more dramatic. The search for natural facial expression is combined with a quest for a symbolism closely related to the content of the delusion. The mouth is either tightly closed or the lips are protruding; there is defense against intrusion of the demons, submission to their will or aggression against them. ("The heads with the protruding lips," the artist said, "should frighten the demon.") I shall be brief here in saying that this second purpose leads us to a function of human expression which is more archaic than art. In the schizophrenic regression, we see the emergence of apotropous masks, which are meant to turn away evil. What has started out as a study in human communication, ends up as a magic performance in which residues of intelligible expression and pure magic are intertwined.

In spite of their delusionary meaning, the busts never lost a certain attraction for the public. In one respect they met with the public's demands. They were produced in a period of transition from the realistic but unruly portraits of the earlier 18th century to the portraits of the 1780's in Greek and Roman style. Even in his psychosis Messerschmitt had not lost contact with the artistic currents of his time. In this respect, his work remained fully integrated. This indicates the limitations of Ego regression in his case.

The third case leads to another extreme. It was studied by Dr. Elsa Pappenheim and myself, in 1938, at the Psychiatric Clinic in Vienna, and will be published in full, shortly.[18] A man in his forties, by profession an architect, a sturdy fellow, had repeatedly been in insane asylums. The diagnosis was paranoid schizophrenia. The patient felt himself to be, at various times, God, and the son of God. His former relationship to a woman from whom he had separated played a part. She was at once Eva and the Virgin Mary.

The delusion was charged with political symbols: the insane was

[18] For a summary see Kris, E., "Probleme der Aesthetik," Internationale Zeitschrift fuer Psychoanalyse und Imago," 1941/2.

a Nazi revolutionary against the Catholics who were at the time dominant in Austria. An accidental circumstance facilitated the investigation. I had met the architect previously, and he remembered me. Since he knew of my interest in criticism of art, he was more willing than he otherwise might have been to discuss what he had recently produced, and what he produced in insanity.

From the study of his work we soon derived the insight that what he drew was not essentially a representation, not mainly an image to be looked at by an audience. The drawings had a different purpose. All of them were signed by the artist and carefully dated. The signature and many other symbols which he used indicated clearly that he who had produced the drawings was God himself, God the artist. However, the signature again was not meant to identify the creator of the work, but rather to make the work effective; therefore, if the effect of the work had to be reaffirmed, the artist used to sign his name to the drawing not once but repeatedly; and, occasionally, when we submitted to him drawings which he had made several years before, he would resign them in order to revalidate them.

The drawings were verdicts. If he drew the tombs of his enemy, the enemy was condemned and killed. If he drew the City of Light, surrounded by angels, it meant the decision that Vienna would become a City of Light, the New Jerusalem, like Berlin, enlightened by the Nazis, the new Rome. One might say that, in these drawings, the artist intended more to reform the world than to depict it.

One of the main symbols he used was a triangle with his—God's —initials written in it. One day, when an attendant had refused him a second cup of coffee, we ourselves watched the use of this symbol in pure magic. The artist showed us a paper napkin on which there was the triangle filled in pencil. We found that he had written down the name of the attendant previously. Then he had heavily crossed out the name. The attendant, he said, was condemned.

The case requires little elaboration. The regression to the phase of omnipotence of thought so frequently found in schizophrenia extends here to the use of symbols. The representation of these symbols becomes magic ritual. What we have seen in the case of Messerschmitt as one tendency which only partly disrupted his work, here becomes the purpose of creation.

Any theory regarding the relation of art to magic, which these case histories might suggest, requires further confirmation. I shall

briefly discuss three areas where I hope that further research will produce relevant data and where some such data has already been obtained. First: Amongst psychotics in asylums there are some who, at the onset of psychosis, suddenly turn to creation. They fill pages with manuscripts or with drawings. No statistical evidence is known to me as to the number of these cases, nor is it ascertained whether all or most of them are schizophrenic. A not-too-cautious estimate published in Germany about twenty years ago mentions about 2% among all the psychotic inhabitants of asylums as subject to "spells of production." I am inclined to think that this estimate is much too high. The relationship between creation and psychosis is sometimes dramatic. Let me quote, as an instance, a case observed in 1932, in Spain. An illiterate agricultural laborer is brought to the asylum and he starts to carve with a piece of glass from the window pane, on the window sill. The doctors supply him with knife and wood and he carves on. After several weeks, when some of his symptoms disappeared, he was turned to garden work as occupational therapy. He exchanged the knife for the spade. The psychiatrists did not carefully watch the transition. After a few weeks a complete remission took place. Before discharge from the asylum, the patient was asked to add to the collection of his works one produced after recovery. He did not recall ever having handled a knife or ever having projected a vision into wood. Urged to do a favor for those who had cured him, all he produced was a barren jug, painfully cut out of a block of timber; whereas in his insanity, he had produced a set of fantastic and highly "symbolic" sculptures.[19]

There are many similar cases described in literature. From experience with some of them I am inclined to believe that the overflowing urge which drives to production is frequently connected with the magic meaning of the product. The insane creates, since the process of production is likely to change, in a magic sense, the world which he depicts or describes. Clearly, this is not a total but only partly an explanation of the phenomenon.

I speak with greater assuredness about the second area of research. It concerns unconscious determination of creation in painting and sculpture. In psychoanalytic observation of several individuals proficient in the arts, I have gained the impression that some of the ele-

[19] Villamil, "Matiz intenso de religiosidad inconsciente del psiquismo humano," Las progressos de la clinica Madrid, 1933, No. 254; Kris, E., "Bildnerei," etc., *loc. cit.*, p. 343.

ments which the psychotic process reveals are, as unconscious determinants, influential in all creation.

Wherever, in psychoanalysis, we observe artists, painters or sculptors, we find that, during creation, a complex process takes place. No model is simply copied. An image is absorbed by the eye and then reprojected. Creation involves destruction; every stroke simplifies something; but every stroke also is part of a new shape and therefore restitutes and constructs. The degree, to which one, the other, or both of these meanings, destruction and reconstruction, predominate, seems to vary. I do not believe that they are ever totally absent. In destroying and rebuilding the world, the full experience of control arises. Control means a supremacy of a God-like nature.[20]

In order to avoid the suspicion of having projected a theory into case histories, I quote from a case history published independently of my observations. A patient of Nunberg's describes the experience while drawing a model, in these terms: "When I draw it I have a feeling of tactile communication, as if every line and stroke palpitated under my hand, and through this process only I learn to understand the essence of the model, in taking it into myself . . . Then it is mine . . . If I want to redraw it, I am independent of the object, since I can reproduce it as frequently as I want; it comes out of myself."[21]

I have, with at least one painter, seen this process in a genetic perspective. As a child of three she started to draw in order to replace a flirtatious mother whom she frequently missed. As a woman of thirty, when a man unworthy of her love was successfully making claims to her emotions, she could free herself from the threat of masochistic submission by painting his portrait. The mechanism she employed in the solution of libidinal conflict is inherent in the process of artistic creation. While we cannot generalize as to details, these and similar observations seem to indicate that magic control through artistic creation is one of the frequent unconscious determinants of production in the arts. In mythology and the folklore of aesthetics, in tales about art and artists, which can be traced back for more than two and a half thousand years, in Western European civilization, for several millenia in Chinese civilization, art and artists are presented as magicians, as rivals of the Gods; and Prometheus, who is nailed to the rock because he competed

[20] For the relation to destruction and reconstruction see Sharpe, E., "Similar and divergent unconscious determinants underlying sublimation in pure art and pure science," International Journal of Psychoanalysis, **16**, 1935, p. 186 ff. See also, Levey, H., "A theory concerning free creation in the inventive arts," Psychiatry, **111**, 1940, pp. 229 ff.

[21] Nunberg, Allgemeine Neurosenlehre, 1932, p. 132.

with the sculptor God in making humans out of clay, is, I believe, the arch type of all artists. They, like him, pay with the pains of creation.[22]

I hope that clinical study in institutions and psychoanalytic observations of artists will make it possible more closely to elaborate on the relation of art and magic. But no stringent confirmation may be expected in the third area, in the history of the arts themselves.

My hypothesis is that one of the roots of art—if we may speak of roots in a concrete sense—leads directly to magical ceremonies. Similar hypotheses have been discussed by various authors. I, myself, have tried to show that at least one branch of art permits us to follow the development from magic to art in concrete detail. I refer to caricatures. Three phases can be distinguished: There is first the magic level, found widely spread over the globe. Some idol representing a person is killed by some manipulation, by putting pins into it. Second: An image of the person is depicted, but the person is depicted as being killed. Capital punishment was painted instead of performed. This usage was current in certain civilizations, if an evil-doer had escaped justice. Third: The evil-doer is abused in image. His face is shown to be similar to that of the devil, and people are supposed to understand that, since he resembles the devil, he is the devil. This third stage is the decisive one, because here communication is made to an audience, which is given something to understand. The artist, instead of magically influencing the work, by executing punishment through sorcery, addresses the community in order to influence their judgment of the portrayed individual.[23]

I here summarize: Regression leads from communication to magic. Progression, according to this hypothesis, leads from magic to communication. Scientifically accessible is the former; and a psychology of art which we may one day obtain, complex as it will be, and based on

[22] Kris, E., "Probleme der Aesthetik," *loc. cit.*; Zur Psychologie der aelteren Biographik, Imago, **21**, 1935, pp. 320 ff.

[23] See Gombrich, E., & Kris, E., "Principles of caricature," British Journal of Medical Psychology, **17**, 1938, pp. 319 ff., where the problem is stated in the following terms:

"In the early primitive stage, the hostile action is carried out on the person through the picture. Every injury done to the picture is thought to touch the person and in reality wound the individual. Picture and person are one, damage done to the picture is damage done to the person. This is the stage of all practised effigy magic; which, of course, no longer exists in this ideal form in our culture.

"In a second stage the hostile action is carried out on the picture instead of on the person, or else the picture is intended to perpetuate in graphic form a hostile action, injury, degradation or shame. It is carried out only on the picture and does not wound the person himself, but only his honour. That is the stage of the defamatory, of shame pictures.

"In the third stage, to which caricature belongs, the hostile action is carried out by altering the portrait only. It remains in the picture, in the aesthetic sphere. This sphere denotes the methods peculiar to caricature. Thanks to the power of the artist the picture is altered through the medium of his art. Man is interpreted in a picture and only this interpretation contains criticism and aggression." (P. 340.)

the cooperation of many schools of psychological thought and on many data to be interrelated. Such a psychology of art might then sharpen our eye for an understanding of art as a phenomenon in the social history of man.

SECTION OF ANTHROPOLOGY

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DOCTOR E. FRANKLIN FRAZIER, Howard College, Washington, D. C.:
*A Comparison of Negro-white Relations in Brazil and in the United States.**

Soon after the turn of the present century, Viscount Bryce expressed the opinion that the close and widespread contact of the advanced and backward races was the completion of a world-process which marked a crisis in the history of the world.[1] The process of which Viscount Bryce spoke had its origin in the economic expansion of western Europe during the fifteenth and sixteenth centuries, which resulted, not only in the spread of European culture, but in the expansion of the white race. Until the fifteenth century, the white race had been confined to western Europe and, only toward the close of the century, was the Moor expelled from the Iberian peninsula. In eastern Europe, the Asiatic still threatened the frontier. But in the fifteenth and sixteenth centuries, the maritime nations of Europe, in their quest for spices and gold, circumnavigated the globe and charted the course of their future colonies. Yet, it was to require two centuries before the colonizing white nations would secure settlements of importance in the newly discovered lands. The discovery and settlement of Brazil by the Portuguese in the sixteenth century, the settlement of North America by the Spanish and French in the sixteenth century, and by the English in the seventeenth century, were phases of the expansion of European peoples.

Although the initial impulse behind the economic expansion of Europe was the search for precious metals, the colonial powers soon began to exploit the productive powers of their colonies. In the tropical regions, the plantation system of agriculture became the characteristic form of industrial organization. In regions of "open resources," the plantation type of exploitation has required some form of forced

* The materials upon which this paper is based were collected during a study of the Negro family in Brazil, made possible through a fellowship grant from The John Simon Guggenheim Foundation for the year 1940-41.

[1] James Bryce, "The relations of the advanced and the backward races of mankind." (Oxford, 1902), pp. 6-7.

labor.[2] In Brazil, as in the southern part of the United States, when the Indian proved inadequate, the African slave trade provided the necessary forced labor. Consequently, in both countries, the question of race relations is concerned primarily with the relations of whites and Negroes.

In order to present an adequate analysis of the differences in race relations in Brazil and the United States, it is necessary to give at least a brief account of the conditions under which the Negroes were introduced into the two countries, their treatment under the plantation system, their emancipation, and their subsequent position in the economy of the two countries. Since Brazil did not offer the ready booty in gold, spices, silks, and precious stones as did the Indies, the Portuguese neglected the country for a quarter of a century. Then, during another quarter of a century, there were explorations along the coast and the country was divided into capitancies, which were held as feudal fiefs of the king of Portugal. During this period, the wifeless Portuguese adventurers began to mix their blood with the Indians and the offspring of such unions provided wives for later settlers. By the middle of the sixteenth century, Thomé de Souza, Captain-General of the colony, had founded the city of Bahia, or Bay of All Saints, which became the center of the African slave trade. Under Souza's administration, the cultivation and manufacture of sugar was begun in the settlements about Bahia. And it was to meet the demand for laborers that Negro slaves were introduced into the colony.

The southern part of Brazil did not develop as rapidly as the northern part during the early years of settlement. Owing to the failure of the cultivation of sugar, only a few Negro slaves were introduced. The colonists developed a sustenance economy, raising grapes for wine, wheat, and tobacco, and carried on a trade in Indian slaves. These settlers mixed freely with the Indians and there grew up in southern Brazil a population that was largely mixed with Indian. By the close of the seventeenth century, it is estimated there were not more than 100,000 people in Brazil who spoke Portuguese and, in this total, the pure whites were a minority.[3] The Portuguese officials constituted an upper caste, ranking higher than the Portuguese born in the colony. Next in rank came the Indian-white mixtures and, just

[2] H. J. Nieboer, "Slavery as an industrial system" (The Hague, 1910), p. 385. "Among the people of the first category (open resources) the means of subsistence are open to all; everyone who is able-bodied and not defective in mind can provide for himself independently of any capitalist or landlord."

[3] Pandiá Calogeras, "Formação Historica do Brasil" 3a, Rio de Janeiro, 1938, pp. 32-33.

below them, the mulattoes. After the mulattoes came various crossings of mulatto-Indian and Negro-Indian mixtures. At the bottom of the social pyramid were the slaves, the red slaves ranking above the black slaves.[4]

Beginning in the middle of the sixteenth century, the Jesuits carried on their missionary work for over a hundred years among the Indians. It was in the mission that the Indian submitted to the discipline of regular labor and ceased his resistance to the Portuguese. In 1570, the Jesuits secured a royal decree abolishing Indian slavery in the Bahia area and, in 1640, this decree was extended to all of Brazil. It has been stated that Negro slavery was introduced into Brazil at the suggestion of the Jesuits in order to save the Indian from annihilation. But, as Gilberto Freyre has pointed out, the Indian was not replaced by the Negro because of moral reasons or because of the Indian's pride and courage, as Indianphiles have said.[5] The Indian was not only physically inferior to the Negro, but he was a hunter and a nomad and in a more primitive stage of culture than the Negro. The Negroes imported into Brazil came from areas in Africa where agriculture and the arts had reached a high degree of development. Therefore, the Negro, because of his cultural heritage, provided the basis of Brazilian civilization in the north.

The number of Negroes imported into Brazil and their distribution in the country were governed by labor demands and the development of agriculture and industry in various areas. The cultivation and manufacture of sugar in the north drew the largest number of slaves. According to conservative estimates, 350,000 slaves were imported in the seventeenth and 1,000,000 slaves in the eighteenth and nineteenth centuries, for the growing of the cane and the manufacture of sugar.[6] Gold was discovered in Minas Geraës toward the close of the seventeenth century and reached its maximum production in the middle of the eighteenth century. For the mining of gold, 600,000 Negro slaves were imported. After coffee production began in 1820, a quarter of a million more slaves were imported. In addition to these importations, a million more slaves were imported for the mining of diamonds, the production of tobacco, cotton, food stuffs, and for domestic and com-

[4] *Ibid.*, p. 33.
[5] Quoted in Roberto C. Simonsen, "Historia Economica do Brasil, 1500-1820," Rio de Janeiro, 1937, 1, pp. 199-200.
[6] *Ibid.*, pp. 201-205.

mercial services. Altogether, according to a conservative estimate, three and a third million Negroes were imported into Brazil.

The type of rural civilization which grew up in Brazil on the basis of African slavery has been described by Gilberto Freyre in his celebrated work, *"Casa Grande e Senzala."*[7] As indicated in the subtitle of this book, slavery became the basis of a patriarchal economy. Under the patriarchal economy, the Portuguese masters and the Negro slaves lived in a type of close and intimate association that excells the most sentimental and romantic accounts of the social solidarity existing between master and slaves in the southern States. The close relationship between the two races in Brazil has been attributed by Gilberto Freyre to the fact that the Portuguese had occupied a marginal position, racially and culturally, between Europe and Africa. Concerning the widespread racial mixture which took place in Brazil, he writes:

> "One should remember that probably, owing to their intimate contact with the Moors, the Portuguese were from the beginning admirers of dark women. Girls, some of them blonde, were sent to Brazil from Portugal—sometimes by the Queen of Portugal herself—to marry Portuguese gentlemen established there as planters. These girls were sent to Brazil, not so much for the sake of race purity, as to preserve the social rank and the aristocratic status of the planters. But personally many of those gentlemen preferred Indian and even Negro and mulatto girls. . . . Some planters were careful in their importation of slaves from Africa to arrange that pretty girls should come. These sexual preoccupations were unworthy of Christian monogamic gentlemen; they show how much the Portuguese had come under the influence of the polygamous Arabs and the Moors. But one should not forget that those same sexual preoccupations led to the formation of a mixed race, as we have seen. Slaves from African groups far advanced in their culture, like the Mohammedan Negroes who knew how to read and write in Arabic, were imported to Brazil. And sexual selection had something to do with the importation of Africans from so fine a stock."[8]

The mixed-blood offspring of the sexual association between the two races enjoyed special privileges and were destined to play an important rôle in the history of Brazil. As the rural patriarchal organization disintegrated and urban communities began to dominate the political as well as the intellectual life of the country, the mixed-blood found an opportunity to compete on almost equal terms with the pure-blooded Portuguese. In a book describing the process by which the urban communities overcame the power of the rural patriarchal organization, Gilberto Freyre has devoted a chapter to the rise of the

[7] Gilberto Freyre, "Casa Grande e Senzala," Rio de Janeiro, 1939. *Casa Grande e Senzala* may be translated as *the "big house" and the slave quarters.*

[8] Gilberto Freyre, "Some Aspects of the Social Development of Portuguese America" in Charles C. Griffin, Editor. Concerning Latin American culture (New York, 1940), pp. 83-84.

bachelor of arts and the mulatto.[9] These two elements in Brazilian society were responsible for its political and economic transformation during the nineteenth century.

The abolition of slavery in Brazil did not occur until 1888, or nearly a quarter of a century after the Civil War in United States. Although, as Ramos points out, abolition came as "the result of a long process of evolution of public opinion,"[10] it was accompanied by heated controversies involving economic interests.[11] However, it was not accompanied by a violent civil war, as in the United States. There was no sharp boundary between free and slave territory, nor was there a well-defined conflict between an industrial and an agrarian economy. All classes were found in the abolition movement, some of the leaders being Negroes and persons of Negro descent. This was an indication of the extent to which Negroes and persons of Negro descent had already been assimilated into Brazilian society. One of the main consequences of abolition for the problem which we are considering is that it increased the mobility of the Negro population and facilitated the process of race mixture.

This brief account of the historical background of race relations in Brazil is sufficient to make apparent a number of important contrasts with the situation in the United States.

In a certain sense, one may not properly speak of race relations in Brazil. For, as Dr. Park has pointed out in an incisive essay, *The Nature of Race Relations*, "Race Relations . . . are not so much the relations that exist between individuals of different races as between individuals conscious of these differences."[12] In Brazil, there is lacking, both on the part of the Portuguese and the "white" Brazilians and, on the part of the "black" or "colored" Brazilians, a consciousness of racial differences. In fact, it is impossible to secure accurate figures on the various racial elements in the Brazilian population. In 1830, it was estimated that about 71 per cent of the population was white and of mixed blood and 29 per cent Negroes.[13] Since the emancipation of slavery in 1888, there has been an unwillingness to separate the population on the basis of race. However, an examination of 30,000 soldiers by an army officer gave the following percentages: whites, 59; mulat-

[9] Gilberto Freyre, "Sobrados e Mucambos," São Paulo, 1936. The title of this book may be translated as *Two-storied town houses and huts.*

[10] Ramos, *op. cit.*, p. 54.

[11] João Dornas Filho, "*A Escravidão no Brasil*" (Rio de Janeiro, 1939), pp. 133 ff.

[12] Robert E. Park, "The Nature of Race Relations," in Edgar T. Thompson, Editor. "Race relations and the race problem," Durham, N. C.: Duke University Press, 1939, p. 3.

[13] Arthur Ramos, "The Negro in Brazil" (Washington, D. C., 1939), pp. 9-10.

toes, 30; Negroes, 10; and caboclos (Indian-white mixtures) 1 per cent. These percentages are different from those obtained by Professor Roquette Pinto who, after researches in the National Museum, arrived at the following percentages for the various ethnic elements: whites, 51; mulattoes, 22; caboclos, 11; Negroes, 11; and Indians, 2 per cent. During his recent study of the Negro in Brazil, Pierson undertook to secure statistical data on race mixture in the city of Bahia.[14] He made an examination of the photographs of the first five hundred persons listed in 1937 as "brancos" or whites in the files of the Gabinete de Identificação.[15] He identified 68 per cent as whites, 19 per cent as mulattoes, 7.8 per cent as of white and Indian ancestry, and the remaining 5.2 per cent of Indian-white-Negro blood. In 1936, he undertook to determine by inspection the racial origin of 5000 participants in a festival in Bahia.[16] He classified 31.7 per cent as European, 18 per cent as Negro, 49.9 per cent as mulatto, and less than one per cent as of Negro-Indian ancestry. The figures are practically the same as those which were furnished me by Dr. Adolfo R. Leite, the statistician in the Department of Health of Bahia, who made his estimates on the basis of the school population and birth and death rates.[17] For the year 1938, he estimated the racial composition of Bahia to be as follows: whites, 32 per cent; colored, 49 per cent; and Negro, 19 per cent. As we shall see, the racial classification has little significance from the standpoint of race relations as we conceive them in the United States. In fact, the figures just cited are not so much an index to the distribution of races in Brazil as an indication of the extent to which a new race is being formed.

In contrasting the racial situation in Brazil with that in the United States, much emphasis has been placed upon the difference in attitudes. This factor was of considerable importance in the early history of Brazil when compared with the traditional racial feeling in the United States. For, even during the early years of the seventeenth century, when Negroes had the same status as white indentured servants, distinctions based upon consciousness of racial differences existed.[18] As the importations of Negroes increased, this racial consciousness increased. This was not only true of the South where in many areas the Negroes out-

[14] Donald Pierson, "Negros in Brazil," (Chicago, 1942).
[15] *Ibid.*, pp. 128-129.
[16] *Ibid.*, pp. 131-132.
[17] See E. Franklin Frazier, "The Negro family in Bahia, Brazil," American Sociological Review. 7 (August, 1942), pp. 467-468.
[18] See Helen T. Catterall, Editor. "Judicial cases concerning American slavery and the Negro. (Washington, D. C., 1926.) I, p. 77.

numbered the whites and there were fears of insurrections. In fact, the intense racial feeling in the South has sometimes been attributed to fear of insurrections. However, in Brazil there were numerous slave insurrections which were attended by greater success than those in the United States. Pierson has suggested that the intense racial feeling in the South is due to the Civil War and, more especially, to the racial conflict during Reconstruction.[19] Undoubtedly, these various cultural and historical factors have played an important rôle in differentiating the course of the development of race relations in the two countries. They should not, however, be regarded as causative factors, but should be considered in relation to more fundamental ecological, economic and cultural factors.

The first fact of importance is the difference in the geography of the two countries. A large part of Brazil, especially the areas into which Negroes were imported, is in the tropics. Although the question has not been settled whether the white man can establish permanent settlements in the tropics, this factor has important consequences for race relations. The Portuguese, who have been regarded "particularly fitted for tropical settlement by environmental experience and racial history,"[20] were confronted, as other white races, with the problem of adapting themselves to life in the tropics. The most successful white settlement, at first by the Portuguese and later by the Italians and Germans, were in the highlands of Brazil. In the northern part of Brazil, where large numbers of Negroes were imported, there was a struggle for existence in a geographical environment in which the Negro had the advantage. The Negro slaves found their new habitat very similar to their homeland, in regard to the climate and the vegetation. It has been suggested by some Brazilian scholars that the ability of the Portuguese to survive in the new environment was due to the admixture of Negro blood. Whether this is true or not, in the southern States the white man was not confronted with the problem of meeting the superior adaptation of the Negro to the geographic environment. If it is true that Negro slavery displaced white servitude because it was more economical, this only emphasizes the fact that social rather than biological factors were responsible for the growth of the Negro population in the South. Even in those States where the plantation system

[19] See Pierson, *op. cit.*, p. 347.
[20] A. Grenfall Price, "White settlers in the tropics," (New York, 1939), p. 15.

has flourished, there has been a decline in the relative numbers of Negroes during the present century.

The next factor of importance in comparing race relations in the two countries is the economic relations of the two races. In Brazil, as in the southern States, Negroes were introduced primarily to provide the labor needs of the plantation economy. But the rôle of the plantation system in the economy of the two countries was different. Up until the nineteenth century, Brazil was essentially a rural civilization resting upon the plantation economy. In the United States, despite the political powers of the planter class, only eighteen per cent of the farms in the South were classified as plantations. Because of the romance that has grown up about the planter class, the white gentry has been absorbed with the "poor whites." During the period of the plantation regime in Brazil, there was neither a white gentry nor a "poor white" class of any importance from the standpoint of numbers. Moreover, the plantations were more self-sufficient than those in the United States. The dependence of the Portuguese upon the labor of the Negro was greater. This dependence was not simply upon the physical strength of the Negro. The Portuguese were dependent upon the technical skills of the Negro. Many of the Negro slaves introduced into Brazil were not only highly skilled craftsmen but were often more literate than the Portuguese. This was especially true of those who had come under Mohammedan influences.

Writing of the "great house" in which the owner led a patriarchal existence, Gilberto Freyre states that it "came to symbolize not only an economic but a social and cultural system. It served not only as the residence of the plantation owner, but as fortress, school, guest house, church, hospital, bank and even harem."[21] This description calls to mind many of the characteristics of the plantation system in the antebellum South. However, in the South, the plantation culture did not dominate society to the extent that it did in Brazil, nor were these various characteristics as highly developed as in Brazil. Although there was a tendency in the South for the plantations to assume a patriarchal character, women were not subordinated to the extent that they were in Brazil, where they "were treated by their husbands in an almost oriental or Moorish way."[22] Moreover, it does not appear that, under the patriarchal regime in the South, there developed the solidarity of

[21] "Some aspects of the development of Portuguese America," p. 83.
[22] *Ibid.*, p. 97.

feeling and sentiment that characterized the patriarchal household in Brazil. This was probably due partly to differences in racial attitudes, but it was also related to greater isolation of the Brazilian plantations. Even in regard to the racial mixture which occurred on a large scale in the ante-bellum South as in Brazil, there were important differences. It is likely, that because of Puritanical mores and the higher status of women, there was a sense of sin and greater protest against concubinage in the South. Attitudes toward racial mixture in Brazil had been influenced to some extent by the fact that many of the early settlers were men without families. They had mated with both Indian and Negro women, and the offspring of these alliances had provided wives for later settlers. On the other hand, the migration of family groups and white women to the colonies in North America tended to preserve the integrity of the white family.

As one studies the history of the relations between whites and Negroes during the plantation and slavery regime in the two countries, other important facts become apparent. In both countries, a class of free Negroes, among whom there were many mulattoes, grew up outside the slave system. In both countries these free Negros and mulattoes had gained their freedom through purchasing it, through the humanitarianism of their owners and, more especially, because their white fathers had set them free. But here the similarity between the situation of the free Negroes in Brazil and the United States ends. The free Negro in the South, and even in the North, was an anomaly. There was no place for him in the economic or social organization. In the North, he led a precarious existence, because European labor was plentiful and could exclude him as a competitor. In the South, especially in the plantation area, the free Negro could find no place in the economic organization and was considered a threat to the slave regime. Hence, free Negroes were concentrated largely in cities which have always provided a refuge for those elements that can not fit into the traditional social organization. In Charleston, South Carolina, and New Orleans, large and prosperous communities of free Negroes, largely of mulatto origin, grew up. Although, economically, they acquired a secure foothold in the economic organization, they constituted a distinct caste in society. Yet the continual sexual association between whites and the mixed-bloods tended to erase the color-line and nullify the attempt to maintain a pure white caste. In Maryland and Virginia, where the ecological basis of slavery had disappeared, a semi-

free class of Negro laborers came into existence. Sexual relations between this class and the whites were likewise making it impossible to maintain a caste system based upon race purity. Moreover, it should not be overlooked that, despite the effort to maintain caste in the southern States, white fathers often gave their children a good education and left them property which enabled them to rise in the world.

Yet, slavery in the United States provided a form of accommodation between the two races that did not exist in Brazil. This was not due entirely to the absence of race feeling in the latter country but also to the existence of a large "poor white" class in the United States. Even during the slavery period, there were protests on the part of the non-slave-holding whites against the monopoly which the free blacks and mulatto artisans had in the economic organization. As long as slavery continued and political power was in the hands of the plantation owners, who often hired out their black mechanics, the protests of the "poor whites" were ineffectual. However, when the slaves were emancipated they were thrown into competition with the "poor whites," who, through organization and a system of apprenticeship, were able to exclude the Negroes from competition. Moreover, when the industrial revolution reached the South, the new forms of industry demanded skilled industrial workers and not workers possessing the skills required in the older handicrafts. The cotton mills, which symbolized the impact of the industrial revolution upon the South, became an exclusive field of labor for the "poor whites."

These economic, political, and social conditions did not exist in Brazil. The transition from slave to free labor caused, to be sure, some dislocations in the economy. However, the abolition of slavery dealt the final blow to the decaying plantation economy and the patriarchal regime. In the shift of political and economic power and cultural dominance to the towns, the educated mixed-bloods, as well as the educated whites, rose to positions of importance.[23] There was, undoubtedly, some prejudice against the mixed-bloods, but they were too numerous and too powerful for the relatively small number of pure whites to exclude them from competing for a place in the political and social organization. Many of the mixed-bloods were officers in the army, which comprised many blacks as well as mixed-bloods. Therefore, the entire structure of Brazilian society, both from a racial and an economic standpoint, was such as to preclude the possibility of a bi-racial or-

[23] Freyre, "Sobrados e Mucambos," pp. 302 ff.

ganization. Moreover, whereas in the United States abolition and the emergence of the non-slave-holding whites tended to sever the sentimental ties that had existed between the two races during slavery, in the Brazilian abolition the bonds that had grown up between the whites and the blacks and the mixed-bloods were never broken. In fact, it was often the colored sons of the aristocracy who won the prizes in the competitive life of the new commercial urban economy.

There is one phase of the contrast in race relations in the two countries that has been neglected until recent years. That is the difference in the extent to which the African culture of the Negro slaves survived in Brazil and in the United States. Within recent years, Professor Herskovits has been engaged in discovering African survivals among the Negroes in the United States.[24] Although, in certain isolated areas as, for example, on the coast of South Carolina and Georgia, a number of linguistic survivals have been discovered,[25] there is practically no evidence that African survivals have influenced the social development of the Negroes in the United States. Even Professor Herskovits, who makes the extreme claims for African survivals, states that "one can set off the United States from the rest of the New World as a region where departure from African modes of life were greatest, and where such Africanisms as persisted were carried through in generalized forms, almost never directly referable to a specific tribe or area."[26] On the other hand, in the case of Brazil, it is not necessary to engage in speculation concerning African survivals. First, it should be noted that the Negroes introduced into Brazil did not suffer the destruction of their social life nor experience the distintegration of their cultural heritage to the same extent as the slaves imported into the United States. Therefore, on the large plantations in northern Brazil and even in the region about Rio de Janeiro, it was possible for them to re-knit the threads of their social life and re-establish their traditional culture. In fact, because of the close commercial relations between Bahia, the chief port of entry for slaves, and Africa, diplomatic relations were established with Dahomey.[27] The most important African culture that took root in Brazil was the Yoruban, and it seems that it absorbed or displaced the Bantu and other cultures. For a time, however, the culture of the Islamized Negroes from the Sudan exercised considerable

[24] Melville J. Herskovits, "The myth of the Negro past (New York, 1941).
[25] *Ibid.*, pp. 276-279.
[26] *Ibid.*, p. 122.
[27] Arthur Ramos, "Las Culturas Negros en el Nuevo Mundo." Version española (Mexico 1943), p. 265.

influence on the slaves and race relations in Bahia. It was the Islamized Negroes who were responsible for the recurrent insurrections in Bahia from 1807 to 1830 and the revolution in 1835.[28] The Islamized Negroes organized communities and established schools and, in fact, possessed more literacy and technical skill than the Portuguese adventurers, criminals, impoverished noblemen and gypsies who settled in Bahia.

During the last decade of the nineteenth century, Nina Rodrigues began his pioneer studies of African cultural survivals in the religious practices and the folklore of the Negroes in Bahia.[29] With the exception of Manuel Querino's study of African customs in Brazil, the scientific study of the Negro did not engage the attention of scholars for twenty years after Rodrigues' work. Around 1926, Brazilian scholars, among whom Arthur Ramos, Gilberto Freyre and Edison Carneiro played leading roles, took up the work of Rodrigues. In their work, they have directed their attention chiefly to African survivals in folklore and religion. However, Freyre has devoted his attention not so much to African survivals among Negroes as the influence of African culture upon Portuguese culture in America. Interest of Brazilian scholars in both of these aspects of the influence of African cultural survivals on Brazilian society indicates their importance in the assimilation of the Negro.

The Negro in Brazil, in contrast to the Negro in the United States, resisted the process of acculturation. The uprisings on the part of the Islamized Negroes were neither a racial conflict nor a revolt against the treatment of their white masters. These revolts were essentially a continuation of the wars of the Mohammedans against Christians. Resistance to Christianity has not been confined to the Islamized Negroes. Islamized survivals in the religious practices of Negroes have almost entirely disappeared, whereas the Yoruban religious conceptions and practices have persisted until the present day. In the candomblés of Bahia and the shangôs in the northeast, many of the African elements have been preserved, though they have become fused with Christian ideas and practices. Before the African communities were broken up, other phases of African traditions and social organization persisted, and the relations between these Negro communities and the Portuguese community were similar to those between immigrant communities in

[28] *Ibid.*, pp. 277-279.
[29] See Nina Rodrigues. "Os Africanos" 2a. (São Paulo, 1935).

the United States and the larger society. On the other hand, because of the advantages that the Negro enjoyed in the struggle for survival in the tropical environment, the Portuguese took over from the Africans many elements of the culture of the latter. African influences are apparent in the language, the diet and music of the Brazilians. The influences are not regarded as quaint or exotic excrescenses but as an integral part of the culture of Brazilian society.

A rough index to the acculturation of the Negro and his assimilation into Brazilian society is provided in the study of the changing color of the population of Bahia and the relation of color to the occupational class structure. Whereas, according to the estimates of the statistician in the Department of Health, the proportion of whites in the population had remained about 33 per cent from 1897 to 1938, the black element had declined from 38 to 19 per cent and the colored element had increased from 29 to 47 per cent.[20] Just as we have seen that the designation "white" in the records of the Gabinete de Identificação did not mean pure white ancestry, the percentages for whites just cited include mixed-bloods. In fact, the term "white" is used so loosely in Bahia, which is called A Velha Mulatta or "The Old Mulatto Woman," that Brazilians often speak of the Bahian whites. The term "white" is closely tied up with the social status, as it has been in the case of mixed-bloods throughout the history of Brazil. Therefore, in considering Pierson's tables showing the proportion of whites, colored, and blacks in the various occupations, this fact should be kept in mind. According to Pierson, "The whites, as might be anticipated, are concentrated in the upper levels. Their numbers, both absolute and relative, diminish sharply as one descends the occupational scale, and they appear only in small percentages in the lower tiers."[21] For example, we find street-sweepers distributed as follows, as to color: mulatto, 62.7 per cent; black, 34.7 per cent; and white, 2.6 per cent. On the other hand, physicians show the following distribution: white, 63.0 per cent; mulatto, 20.0 per cent; black, 1 per cent; and branco da Bahia or Bahian white, 16 per cent. Pierson is correct, in my opinion, in ascribing the situation to the fact that the blacks have not been free as long as the mixed-bloods. By that statement, he means, of course, that because of their short period of freedom, their poverty, and their lack of

[20] See Diagram 1, in Frazier, "The Negro family in Bahia, Brazil," *loc. cit.*, p. 467.
[21] Pierson, *op. cit.*, p. 178.

education, the blacks as a group have not been able to compete with those of a lighter complexion.

But it would be wrong to assume that, during the process in which the Negro and the mixed-blood have been assimilated into Brazilian society, or even today, race and color consciousness on the part of the various racial elements has been entirely absent. I think the process of assimilation may be shown in a study of the lives of Machado de Assis and André Rebouças. The first was, perhaps, the greatest writer Brazil has produced, and the second, a distinguished engineer and an abolitionist. Machado was born in 1839 of poor mulatto parents, both of whom were free, his father being a house painter and his mother a laundress.[32] He was taught to read and write by his stepmother and later he secured a job in a printing office. When he went to Rio de Janeiro he entered upon the career of a journalist. For a brief period his creative efforts were in the field of poetry. But he soon turned his attention to the romantic novel, a field in which he established himself as a master of style and the author of the literary emancipation of Brazil from Portugal. Rebouças was born a year before Machado, of parents who had a small admixture of white blood.[33] His father was a lawyer and a provincial deputy who, because of his opposition to the smugglers of slaves in the State of Bahia, was forced to move to Rio de Janeiro. Young Rebouças, as well as his brother, received a good education in engineering and was sent abroad by his father to complete his education. He served as an engineer in the army during the war with Paraguay and surveyed the ground for the first important railways in Brazil. After an active career as an abolitionist, he followed the royal family into exile.

The first fact that strikes us in the careers of these two men is that they were able to compete on almost equal terms with white Brazilians. I say "almost equal terms," because Rebouças complained in his biographical notes that he did not receive the traveling fellowship to study in Europe because of his color. Yet, Machado was given opportunity to participate in the literary movement of his day and Rebouças was given every opportunity to utilize his education and talents as an engineer. Both of these men thought of themselves as Brazilians, though both had some consciousness of their racial origin. Machado was conscious of his mulatto origin and disliked any reference to it. More-

[32] Lucia Miguel-Pereira, "Machado de Assis" (Rio de Janeiro, 1939).
[33] Ignacio Jose Verissimo, "Andre Rebouças" (Rio de Janeiro, 1939).

over, when he began to court the daughter of an aristocratic Portuguese family, there was considerable opposition on the part of her family. Yet, after they were married, they were accepted into the highest social and literary circles in Brazil. Despite the consciousness of their racial origin, they were not "marginal" men or cultural hybrids in the sense in which Park defined the term.[34] The reason for this was doubtless that there was no segregated colored community with which they could de identified. This had important consequences for their work as well as their personalities. Neither of these men were concerned with the race or color problem. Their work was a part of the culture of the community and it was evaluated in terms of the standards of the Brazilian culture. These men were not considered a "Negro writer" and a "Negro engineer," but Brazilian workers in their various fields. This is quite different from the situation in the United States, where there are Negro writers, journalists, and even biologists and chemists and a different standard for evaluating their achievements.

There is, in Brazil, little discussion of the racial or the color situation. It appears that there is an unexpressed understanding among all elements in the population not to discuss the racial situation, at least as a contemporary phenomenon. Apparently, there is a general recognition that Brazil is essentially a country of mixed-bloods and that a new ethnic type is being formed. Oliveira Vianna is a rare exception among the scholars, in that he regards both Indians and Negroes as inferior races and believes that, through Aryanization or a whitening process, Brazil will become a white nation.[35] Although there is no race problem in Brazil, the upper classes are conscious of color differences and these color differences become the basis of social distances that are maintained by a subtle system of etiquette. In fact, these distinctions would escape the casual observer and, even when one discovers them, it is dangerous to generalize about them. If one should suggest an American situation analogous to the situation in Bahia, one might point to the colored community in Charleston and New Orleans, forty or fifty years ago. Bahia is essentially a mulatto community where persons of light complexion tend to dissociate themselves as much as possible from those of dark or black complexion. Of course, this does not preclude friendships between whites and blacks. According to a lawyer of pure Portuguese ancestry, members of his group

[34] See Robert E. Park, "Human Migration and the Marginal Man," The American Journal of Sociology, **33** (1928), pp. 881-893.
[35] Oliveira Vianna, "Evoluçao do Povo Brasileiro" (Rio de Janeiro, 1933).

are likely to be more free and friendly with blacks than the mixed-bloods. It is not unusual for whites or so-called whites to marry people of light-brown and brown complexion. In such cases, the brown-skin people are likely to become essentially white persons, or at least the children of such unions would be regarded as white. In fact, one of the reasons that it is impossible to draw a color line, not only in the north but in other parts of Brazil, is because it would cut across families.

The prejudice toward black persons seems to operate most strongly in intimate social relations involving marriage and in the new type of social life which is developing in clubs and hotels. For example, black persons do not attend the weekly dances at the large hotels patronized by Brazilian officials and business men as well as foreigners; nor are black men to be found at the tennis clubs and the yacht clubs. They may attend on some special occasion, but they do not move about freely and they would not be invited to become members. Of course, if they marry a white person, their children would be eligible for membership provided their parents belonged to the upper economic classes. Since foreign whites frequent the hotels and the clubs, it may be asked, to what extent are they responsible for these attitudes toward the blacks? Undoubtedly, the British and the Americans would not care to have black people in these places and some Brazilians are sensitive to their attitudes in regard to blacks. But the foreigners are not entirely nor primarily responsible for the attitudes toward blacks. They reflect the attitudes of the mixed-bloods who seek to identify themselves with the whites. What has been said so far applies to the upper social and economic strata in Bahia. Color distinctions and prejudices against the blacks are seemingly absent on the whole from the mind of the masses. This is apparent not only in their everyday activities but in the numerous festivals where all colors mingle freely. In fact, it is among the laboring masses that race mixture is continuing on a large scale in Brazil.[36]

In the states São Paulo, Santa Catharina, and Paraná in southern Brazil, color prejudice is much more marked than in the north. These are the states to which have come large numbers of European immigrants—chiefly German and Italian—who have a different attitude toward the Negro from that of the Portuguese. In these areas, the Negro, especially the dark or black person of Negro descent, has be-

[36] Freyre, "Some aspects of the social development of the Portuguese in America," pp. 102-103.

come a conspicuous minority in a predominantly white population. In this region, the attitude toward the Negro assumes often the character of race prejudice as opposed to color prejudice. Although there are no legal discriminations against persons of Negro blood, they are isolated and discriminated against in subtle ways. From conversations with some of the leaders among the blacks, it appeared that the Negro suffers chiefly from the economic competition of the European immigrant, especially the Italian. As the Negro is pushed down in the economic scale, he is unable to acquire the education and the skills which would enable him to compete successfully with other groups. The only escape for the Negro is to mingle his blood with that of the whites. This he is doing, though not as freely as in other parts of Brazil.

Because of the isolation of the Negro in the south, a number of Negro organizations have come into existence to fight discrimination on the basis of color. Out of these various organizations has come *Frente Negra Brasileira,* which was started in 1931 as a movement to include all Negroes in Brazil.[37] In this year, the organization announced, at a meeting of over a thousand Negroes, its program for the improvement of the moral, educational, economical, and political status of the Negro. It was recognized by the government as a political party but, after the changes in the government in 1937, it retained only the cultural and social features of its program. Besides the *Frente Negra Brasileira,* a number of other Negro clubs and associations have been organized in the south and in other parts of Brazil. The organizations in the south are sharply differentiated from those in the north. In the south, they are fighting discrimination and are seeking to integrate themselves into the social and economic organization. On the other hand, in the north, they have cooperated with whites in studying the cultural contributions of the Negro and have fought for religious liberty for Negro cults, as well as the improvement of the social condition of blacks. It appears that the Negro organizations in Brazil lack the drive and motivation of similar organizations in the United States. This is doubtless due to the fact that racial discrimination is not as strong even in southern Brazil as in the United States. In São Paulo, where a number of these movements originated, there are two Negro professors on the law faculty of the University. It appears that amal-

[37] See Ramos, "The Negro in Brasil," pp. 167-176, concerning the various Negro organizations and movements in Brasil.

gamation will constantly undermine these Negro movements unless outside influences affect present tendencies.

Outside influences certainly have had some effect upon attitudes toward colored and black people. Many Brazilians are conscious of being regarded as a colored nation by Argentina and other so-called white nations of South America. Then, in recent years, Nazi racial theories have had some influence among the Germans in the south who tended to intermarry with the Brazilians.[38] The most important source of outside influence has been the financial and industrial penetration of the country. The British and the Americans draw a color line, not only in their social contacts with Brazilians, but when their business houses employ Brazilians as white collar workers. Americans who have gone to Brazil as technical advisers have insisted that even distinguished black officials be ejected from hotels and, when their wishes were not respected, they have left the hotels. In spite of the "good neighbor" policy, it is likely that increasing financial and industrial penetration of Brazil by Americans will accentuate discrimination on the basis of color. Even at the present time, Brazilians are careful to select pictures of the right complexion for the American public.

In spite of these differences between the racial situation in Brazil and the United States, it should be pointed out by way of conclusion that the development of race relations in the two countries reveals some underlying similarities. In both countries, the close association of the whites and blacks produced a class of mixed-bloods. Although, in the United States, an attempt to maintain a caste system has prevented the identification of the mixed-bloods with the whites; through the process of "passing," persons with Negro blood have passed into the white race. With the increasing mobility of our population, it is likely that this will continue. It is hardly probable that the so-called "race purity" laws of Virginia and Georgia will stop the process. Race mixture in the United States, as in Brazil, has been one of the chief factors in the social differentiation of the non-white population, and it has facilitated the social mobility of colored individuals. The relation of color differences to occupational structure in Brazil closely parallels the same phenomenon in the segregated Negro community in the United States. Moreover, as in Brazil, this phenomenon in the United States is a rough index to the process of acculturation, though it does not lead

[38] Emilio Williams, "Assimilação e Populações Marginais no Brasil," São Paulo, 1940, pp. 76-166.

to complete assimilation, because of the attempt to maintain a racial caste in the United States. As the attempt to maintain a caste system becomes less effectual because of urbanization and the general educational and cultural development of the Negro, it is likely that the racial situation will approximate the situation in Brazil.

NEW MEMBERS

ELECTED APRIL 27, 1944

SUSTAINING MEMBERS

Auchincloss, Hugh, M.D., Surgery, Professor of Clinical Surgery, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Barzilai, Gemma, M.D., Pathology, Specialist in Obstetrics and Gynecology, New York, N. Y.

Robinson, Cortland O., M.D., Medicine, Physician, Pan American Airways, New York, N. Y.

ACTIVE MEMBERS

Adams, Mildred, Ph.D., Biochemistry, Research Chemist, Takamine Laboratory, Clifton, N. J.

Bainbridge, William C., B.S., Coloring Matters, Technical Director, H. Kohnstamm and Company, Brooklyn, N. Y., by invitation.

Bell, Paul H., Ph.D., Chemistry, Research Chemist, American Cyanamid Company, Stamford, Conn.

Berk, Bernard, Ph.D., Chemotherapy, Lawrence Richard Bruce, Inc., Stamford, Conn.

Bram, Joseph, Ph.D., Anthropology, Instructor in Anthropology, Queens College, Flushing, N. Y.

Bull, Nina, Biological Psychology, Research Associate in Psychiatry, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Charipper, Harry A., Ph.D., Biology, Professor of Biology, Chairman of Department, Washington Square College, New York University, New York, N. Y.

deGara, Paul F., M.D., Bacteriology and Immunology, Instructor in Pathology, Cornell University Medical College, New York, N. Y.

Friedeman, Ulrich, M.D., Bacteriology and Immunology, Chief of Division of Bacteriology, Brooklyn Jewish Hospital, Brooklyn, N. Y.

Greep, Roy O., Ph.D., Biology, Associate in Pharmacology, Squibb Institute for Medical Research, New Brunswick, N. J.

Hand, George F., Ch.E., Microchemistry, Research Chemist, Sperry Gyroscope Company, Brooklyn, N. Y.

Molitor, Hans, M.D. (Vienna), Pharmacology, Nutrition, Therapeutics, Director, Merck Institute for Therapeutic Research, Rahway, N. J.

Orbach, Fred., Ph.G., Physical Chemistry, Pharmacist, New York, N. Y.

Reynolds, Samuel R. M., Ph.D., Gynecic Physiology, Captain, Army of the U. S., Research Associate, Carnegie Institution of Washington, Johns Hopkins University, Baltimore, Md.

Roberts, Alpheus J., Aircraft Engineer, Owner and Operator, Engine Service and Manufacturing Company, Monroe, Mich.

Rosenfeld, Capt. Morris, M.D., Pharmacology and Experimental Therapeutics. Now—Captain, Army of the U. S. (on leave); Associate in Pharmacology and Experimental Therapeutics, Johns Hopkins School of Medicine, Baltimore, Md.

Schneider, Theodore I., C.P.A., Ichthyology, Osteology, Surgery, Private Research, New York, N. Y.

Sharp, D. Gordon, Ph.D., Biophysics—Animal Virus Research, Associate Biophysicist, Department of Surgery, Duke University Hospital, Durham, N. C.

Smith, Arthur Leo, M.S., Inorganic Chemistry, Research Associate, Polytechnic Institute of Brooklyn, N. Y.

Thompson, Kenneth Wade, M.D., Surgery, Endocrinology, Assistant Professor of Surgery, Yale University School of Medicine, New Haven, Conn.

Tyler, H. Y., M.D., Medicine, Pathology, Instructor in Pathology, New York Medical College, New York, N. Y.
Umberger, Jacob Quentin, B.S., Physical Chemistry, Research Physical Chemist, Columbia University, New York, N. Y.
Watkins, William Way, Ph.D., Chemistry of High Polymers, Research Supervisor, Rayon Department, E. I. DuPont de Nemours & Company, Buffalo, N. Y.
Weider, Arthur, A.M., Clinical Psychology, Research Psychologist, O.S.R.D., Cornell University Medical College, New York, N. Y.
Worley, Leonard George, Ph.D., Cytology, Associate Professor of Biology, Brooklyn College, Brooklyn, N. Y.

ASSOCIATE MEMBERS

Decker, Fred J., M.S., Psychology, Administrative Assistant, New York State Teachers Retirement System, Albany, N. Y.
Doutt, J. Kenneth, M.A. Mammalogy, Curator of Mammals, Carnegie Museum, Pittsburgh, Pa.
Downes, Alfred W., Ph.D., Synthetic Resins, Assistant Department Head, Process Development Laboratory, Carbide and Carbon Chemicals Corporation, South Charleston, W. Va.
Harker, David, Ph.D., Physics and Chemistry, Research Metallurgist, Research Laboratory, General Electric Company, Schenectady, N. Y.
Ives, Sumner Albert, Ph.D., Biology, Head of Biology Department, Professor of Botany, Furman University, Greenville, S. C.
Royer, George L., Ph.D., Physical Chemistry, Assistant Director of Physical Chemical Research, Calco Division, American Cyanamid Company, Bound Brook, N. J.
Sack, Henri S., D.Sc., Applied Physics, Research Associate, Cornell University, Ithaca, N. Y.
Schubert, Albert E., Ph.D., Chemical Engineering, Chemical Engineer, Research Laboratory, General Electric Company, Schenectady, N. Y.
Sinness, Lester S., Ph.D., Industrial Chemical Research, Director, Viscose Rayon Research, E. I. DuPont de Nemours & Company, Wilmington, Del.
Stucklen, Hildegard, Ph.D., Spectroscopy, Associate Professor, Head of the Department of Physics, Sweet Briar College, Sweet Briar, Va.
Turkevich, John, Ph.D., Physical Chemistry, Assistant Professor, Chemistry Department, Princeton University, Princeton, N. J.
Wannier, Gregory H., Ph.D., Physics and Chemistry, Lecturer, State University of Iowa, Iowa City, Iowa.

STUDENT MEMBERS

Solomon, Nathan A., B.S., Biochemistry, Student, Polytechnic Institute of Brooklyn, Brooklyn, New York; Chief Administrative Technician, Jewish Hospital, Brooklyn, N. Y.
Stein, Marcella, Psychology, Junior Psychologist, Brooklyn College, Brooklyn, N. Y.